

October 28, 2006

Professor Patricia Yancey Martin  
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Dear Pat (and Emily, John, and Kathryn),

Ahh, the life of a journal editor is not an easy one; neither is that of an author. I have now read the two reviews of your article, "Adolescent Girls' Race/Ethnic Status, Identities, and Drive for Thinness" and the article itself. As sometimes happens in a diverse social science, they could hardly be more different. One reviewer recommends that we accept the article pending minor revision, the other strongly urges rejection. So it goes. Both of these are competent scholars, although perhaps neither one is the perfect reviewer for an ADDHealth study. They occupy different spaces through their backgrounds and methodologies.

And me? I rather like the article, but it is far enough from my area of expertise that I couldn't provide you the precise suggestions that you will need. (With additional substantive advice, I am confident that I can make a responsible decision). Given that one reviewer says that nothing needs to be done and the other says that nothing can be done, I need to figure out what should be done. They are not quite so extreme, but you get the point.

One possibility is to give you an R&R, sending the paper to new reviewers (because it wouldn't help to send the paper to either of these reviewers again). That would be the equivalent of kicking the can down the road.

I am very aware that you have now been waiting over three months for a response, a delay which, as you know, is a result of the strains on the Tampa office.

Here is what I propose. I will send the paper to at least two other readers, but keep as tight a timeline as possible. I would like another two readers with substantive knowledge of the area, at least one of whom should be familiar with the ADDHealth data or similar methodologies. When the reviews are returned, I will make a quick decision. If it is an R&R, it will be because I will have gained a clear sense of what needs to be done to fix the paper for publication. Of course, the additional reviews may suggest acceptance or rejection.

I do admire how you situated the problem of the article, seeing the body as situation. It reveals that an interpretivist approach can be consistent with systematic data collection. I found the set-up clear and the data presentation effective, but I need a little more help from my friends.

What I need from you right now is an electronic copy of the manuscript, if you are willing to have it sent out for electronic review. (Send to [spq@northwestern.edu](mailto:spq@northwestern.edu)). I keep on top of the progress of the manuscript.

Warmly,

Gary Alan Fine  
Editor, Social Psychology Quarterly  
John Evans Professor of Sociology  
Northwestern University

February 23, 2007

Ms. Emily Boyd  
Department of Sociology  
Florida State University  
Tallahassee, FL 32306

Dear Emily:

The time has come to make a decision on your manuscript, “Adolescent Girls’ Race/ethnic Status, Identities, and Drive for Thinness.” I apologize for the time that it has taken to provide an outcome. I found it hard to find the right reviewers who had the time to provide advice. It happens. Except in unusual cases, I now require three reviewers from the outset and in papers, such as yours, which are outside of my own research and methodological expertise, I request advice from a Deputy Editor.

When I receive a manuscript, I first read the reviews to gain a sense of those issues that the reviewers consider important, then I read the article, and then I read the reviews again. We now have four reviews of your manuscript and a metareview from a Deputy Editor. The original two reviewers were split in their views (we send them to you as PDFs). For the other two reviews, I asked a scholar who works with ADD Health data and one who works on issues of weight. The Deputy Editor works in a related area. In total, you have received many good suggestions from knowledgeable scholars.

As you will read, the three new reviewers advise against publication. I don’t retreat from my previous assessment in my letter of October 28, 2006, admiring your attempt to treat the body as situation, and constituting the process as a “drive for thinness.” However, the new reviewers felt that the literature review was blurred and didn’t provide sufficient structure. They also had complaints about choices of data analysis. In rereading the set of reviews, I feel that it is appropriate to accept the preponderance of advice and reject the article. Given these assessments, a Revise-and-Resubmit at SPQ would not be warranted and not likely to provide a happy outcome.

However, I find that it is striking that the two qualitative researchers (reviewer B from the first round and me) like the article more than the quantitative researchers. What does this suggest? It suggests that there is a core of an idea that deserves expression and emphasis by reframing. Perhaps we were taken by the idea that identity work is important for weight decisions and that the body needs to be considered in a meaning-context. Weight is not a thing, but a situated achievement. The body is not an object but a persona.

I advise you to take all of these assessments seriously and to treat them as the basis of a revision for another journal. Despite their criticism, the issues they addressed focused on strategies of analysis and presentation, rather than on the data itself. My gut feeling – admittedly from a different standpoint – is that a serious revision could well find a home in the sociological literature.

I appreciate your patience in this process. I wish that the result had been more favorable.

Warmly,

Gary Alan Fine  
Editor, Social Psychology Quarterly  
John Evans Professor of Sociology  
Northwestern University

### **REPORT TO THE AUTHOR(S)**

Ms. NUMBER 06-048 – Reviewer A

**TITLE: Adolescent Girls' Race/ethnic Status, Identities, and Drive for Thinness**

This paper uses the well-known and high quality AddHealth dataset to examine to the role that race/ethnicity (and to a somewhat lesser extent social class) plays in girls' perception of being overweight and then what they might try to do about it. The authors (they use the plural) cite a number of studies to back-up their claims and expectations throughout the first half of the paper. Although I am by no means an expert on weight issues, I found the arguments presented and claims made rather obvious. By obvious, I do not necessarily mean convincing or based on good science. It seemed that much of what the authors argued is fairly commonplace, and sometimes rather ideological in nature.

My biggest criticisms of the paper are fourfold: (1) As a purely structural matter, the paper is extremely repetitious and somewhat disorganized. Although the writing in isolation is clear, the authors pose hypotheses, then introduce new empirical literature,

then make emphatic statements of fact, then add a research question or two, then introduce new empirical literature again, then pose more hypotheses, etc. (2) In addition, the authors provide an encyclopedia of empirical and more literary studies, but provide almost no integration or synthesis. Moreover, the way the authors write, especially the front half of the paper, it reads as if they already made up their minds and know the answer to their questions. (3) Related perhaps to the lack of synthesis and integration, is the fact that the study is nearly devoid of theory. This hole, in my view, raises the serious question of what, exactly, is at stake social psychologically? It also begs the questions: Why should social psychologists in general be interested in this study, but especially readers of *SPQ*? (4) The methods used (logistic regression), while perhaps okay in some respects, seem less than optimal for the authors' stated goals. Namely, on page 1, and elsewhere, their stated thesis is that "girl's feelings about and evaluations of her body reflect her race/ethnic (and gender and class) *identity(ies)* relative to her physical body which in turn affects the *practices* she undertakes to control (or not) her weight" (page 1, second full paragraph). This, to me, suggests a rather straightforward causal diagram. By "stacking" race/ethnic groups and social class categories in the same model, direct parameter comparisons (via appropriate statistical tests) could be performed. At the very least, a structural equation model could be added to the study.

Some other observations and criticisms are offered.

1. Identities are evoked, which would lead one to expect some contextualizing within the identity theory literature, but, alas, no theoretical framing is offered.
2. Many of the footnotes are either unnecessary (e.g., 4, 8-10, 16), or could be folded into the text (e.g., 2, 3, 7, 20).
3. Footnote 5 seems to argue against the authors' race/class based differences.

### **Social Psychology Quarterly – Reviewer B**

"Adolescent Girls' Race/ethnic Status, Identities, and Drive for Thinness".

The authors need to be clearer as to how this paper contributes to the existing literature. While whether race affects a girl's feelings of thinness and her usage of no, healthy, or unhealthy weight control behaviors is an interesting and important question, I am unclear as to how the authors of this paper add to the literature that they cite that apparently has analyzed these very questions. I would suggest that the authors need to relate the information in the paragraph beginning at the end of page four and continuing onto page five with specific existing literature. For example, the first "contribution" seems to have already been addressed in literature (as the authors themselves say).

I focus the majority of my review on the methodological adequacy of the research.

An overall comment for the methods section of this paper: The rule of thumb for methods sections is that any reader should be able to replicate the study exactly.

Dependent variable: The author needs to justify coding “perceptions of overweight” into a dichotomous variable. Specifically, I am concerned with grouping girls who report themselves to be underweight with girls who say that are the right weight.

Sample: I am also unclear about the sample used in this paper; how/why the authors eliminated cases should be in the main body of the text and not a footnote.

Family support variables: Imputing missing cases with the value of zero for a variable on a 1-5 scale makes absolutely no sense. The authors are imputing a value that has no meaning on their scale (the model, however, reads it as a real value on the scale – a very extreme value given that their mean is 4.4). If this is not a typo, another method must be used to deal with the missing data. (Checking to see if those cases are significantly different in impacting the dependent variable with dummy variables does not cover this error).

Friendship ties variable: I do not understand how the authors use this variable in their data. They get a one if they have any friends? More than one?

School activity variable: It strikes me that certain sports activities may have a unique impact on perception of being overweight and practices for maintaining/losing weight. Perhaps the authors could address this in their analysis?

Maturation status: Using a self-perceptive measure about body to predict another self-perceptive measure about body strikes me as problematic. The Add Health data has age at first menses available – perhaps that would be a better control for maturation.

Father and mother’s occupation: How do they measure professional? Is it a variable in the Add Health Data? Again, I need to be able to replicate research.

Use of robust standard errors: This is necessary, but the authors should be clear in the text that it is actually only adjusting for one level of nesting.

Results section: The first paragraph of the results section should probably be rewritten for clarity. When reporting about the differences between the groups, the authors do not mention significance. However, Table 1 suggests (I say suggests because I can’t quite tell) that there are no significant differences between groups. This is important. Significance differences should be as clear in Table 1 as they are in the remaining tables.

From the sentence “Analyses not shown indicate that African American girls also have significantly lower odds of perceiving themselves as overweight than do Asian American or Hispanic girls.” I gather that the authors switched out the reference category (whites) in the models and used African Americans as the reference category. Was the same thing done for Asian American or Hispanic girls? Were there significant differences between those two groups?

The notes for Table 1 are incomprehensible. I do not think the percent change columns in Table 2 are necessary.

Describe what Figure 1 represents in more detail.

Minor points:

“Far more girls in our sample admit to using weight control practices than view themselves as overweight, suggesting that claiming to do the practices, for instance exercise or exercise-plus-dieting, is normative for some girls”. Pg. 11. This isn’t necessarily true. One can use a weight control practice to maintain a weight that he or she does not perceive as overweight.

“Although African-American women and girls ...” Pg. 12 there should be no “although” in this sentence and there should be an “and” rather than a comma.

Pg. 14. “We anticipate that this pattern is primarily a characteristics of White girls.” Why?

Pg. 24 The authors can’t really speak to “weight gains” since they are not using longitudinal measures of BMI.

### **Metareview**

Ms. Number: 06-048

Title: Adolescent Girls’ Race/ethnic Status, Identities, and Drive for Thinness

We received four reviews for this manuscript: two during the time it was housed at the University of South Florida and two more after it was transferred to Northwestern. Of the two initial reviews, one was positive and one was negative. Of the two new reviews, both are negative. I will confess to being more persuaded by the negative than the positive reviews, particularly since the positive review identified very serious limitations in the analysis which belie the recommendation.

Rather than repeat concerns expressed in the reviews, I will try to impose some order on them in my comments. As I read the reviews, they identify several limitations to the manuscript. Chief among these is the authors’ failure to carefully situate the analysis in the existing literature. Reviewers commented on the encyclopedic, non-integrative character of the literature review, the lack of a clear theoretical rationale, and the need to be more specific when describing past research. I agree that the specific contributions of the analysis get lost in the interesting, but also unfocused, discussion of the “drive for thinness.” Readers would benefit from a more linear presentation: what do we already know about the association of race/ethnicity with weight, perceptions of overweight, and weight control practices? What new evidence does this manuscript offer? It appears that we already know that African American girls are more likely than White girls to be overweight and that they are less likely to perceive their weight as a problem. (Melissa Milkie’s work is relevant here.) Is the unique contribution of this analysis to show that

African American girls are also less likely than White girls to exercise and diet? Frankly, I would be surprised if someone has not already shown that but, even if they have not, that does not seem like big enough “news” for *SPQ*.

The reviewers’ comments about the literature review take on heightened importance because the literatures that the authors review most thoroughly—writings on body projects, the drive for thinness, and identity—bear only a weak relationship to their analyses. I find the notion of the “drive for thinness” compelling. I agree that girls’ (and, indeed, everyone’s) feelings about their bodies reflect their identities and are socially determined. Where things start to come apart for me is in the analysis. The authors’ central thesis (p. 7) is that “(t)he features of bodies that members of her race/ethnic group value affect her perceptions about and evaluations of her own body and they become part of her identity.” But the analysis does not include indicators of what members of different race/ethnic groups value in bodies or of body-related identities. If, as the authors find, members of different race/ethnic groups have different likelihoods of exercising, dieting, etc., can we really conclude that the differences result from different body-related values and/or body-related identities in those groups? Are there really no other possible explanations? Similarly, if girls exercise to maintain their weight, can we unequivocally consider that evidence of a “drive for thinness”? Can we know for sure that they are performing “an identity that...reflects [their] race/ethnic status”? Could we not see weight maintenance as a healthy behavior, especially for girls who are overweight? I am sure that health researchers would see African American girls’ relative lack of exercise as a problem, not as a sign of their willingness to resist the “drive for thinness,” especially in light of the high rates of obesity and obesity-related complications (e.g., diabetes, heart disease) among African American women.

The slippage between authors’ theoretical orientation and their data is especially striking on p. 11. On this page, the authors argue essentially that girls’ reports of their weight control practices cannot be taken at face value—girls may say that they exercise even when they do not, they may say that they do not exercise even when they do. Fair enough—all survey indicators have some measurement error. But the authors cannot have it both ways—either the data can be believed, in which case the analysis tells us something meaningful about the body projects in which girls engage, or the data cannot be believed in which case the analysis tells us something about the norms of the groups to which girls belong (although it may, then, tell us nothing). In short, I see a fundamental mismatch between the data the authors have and the kinds of statements they want to make. (I might add that the strident tone the authors adopt will turn away some readers who might otherwise be receptive to their arguments.)

The two most recent reviewers question some of the authors’ methodological decisions. To these questions, I add a few of my own. What is the justification for dichotomizing perceptions of overweight? The coding throws away a lot of information. Whose report was used for the measures of parents’ education and occupation—the child’s or the parent’s? How were missing data on these and other variables handled? (I agree with the reviewer who found the handling of missing data on parental support peculiar.) Why did the authors choose not to use the survey estimation procedures that are recommended by

the Add Health staff? The footnotes in Table 1 do not appear to be there. I don't think the coefficients in Table 2 can be interpreted accurately without all of the interactions included. I strongly discourage the authors from reporting .10 significance levels in a sample as large as this. Did the authors estimate interactions between race/ethnicity and social class? The possibility of interactions is suggested by their literature review. Why did the authors choose not to estimate interactions between race/ethnicity and BMI in Table 3 when they had estimated those interactions in Table 2?

In summary, although I am sympathetic to the authors' arguments, I cannot recommend that this manuscript be published in SPQ. It does not appear to make a strong empirical or theoretical contribution to this literature.