Table 1. Coefficients for Stepwise Regression with Hostility towards Whites as Dependent Variable, Northern Black Sample, 1968

<table>
<thead>
<tr>
<th>Variable</th>
<th>Multiple R</th>
<th>R-Square</th>
<th>Change</th>
<th>Simple R</th>
<th>B</th>
<th>Beta</th>
</tr>
</thead>
<tbody>
<tr>
<td>Personal Uncertainty</td>
<td>.228</td>
<td>.052</td>
<td>.052</td>
<td>.228</td>
<td>.436</td>
<td>.235</td>
</tr>
<tr>
<td>System Unresponsiveness</td>
<td>.271</td>
<td>.073</td>
<td>.021</td>
<td>-.099</td>
<td>-.489</td>
<td>.333</td>
</tr>
<tr>
<td>Political Cynicism</td>
<td>.348</td>
<td>.120</td>
<td>.047</td>
<td>.149</td>
<td>.453</td>
<td>.218</td>
</tr>
<tr>
<td>Personal Political Efficacy</td>
<td>.390</td>
<td>.152</td>
<td>.032</td>
<td>.164</td>
<td>.342</td>
<td>.193</td>
</tr>
<tr>
<td>Government Trust</td>
<td>.405</td>
<td>.163</td>
<td>.011</td>
<td>.110</td>
<td>.187</td>
<td>.113</td>
</tr>
<tr>
<td>Status Uncertainty</td>
<td>.407</td>
<td>.165</td>
<td>.001</td>
<td>.014</td>
<td>.161</td>
<td>.080</td>
</tr>
<tr>
<td>Relative Deprivation</td>
<td>.408</td>
<td>.166</td>
<td>.001</td>
<td>-.032</td>
<td>-.086</td>
<td>-.051</td>
</tr>
<tr>
<td>(Constant)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>1.186</td>
<td></td>
</tr>
</tbody>
</table>


Rejoinder

TO THE EDITOR:

Thank you for the invitation to respond to the communication by Miller and Bolce. I shall divide the reply into three sections: (1) restatement of my general position, (2) examination of a series of specific issues, and (3) musings.

**General Issues**

Judging from the first paragraph of their communication, Miller and Bolce did not clearly understand my central point. Allow me to reiterate it here. The point of my article (Crosby, 1979) was that Miller, Bolce, and Halligan (1977) were not logically correct in rejecting relative deprivation theory as the explanation of black urban unrest on the basis of their data and analyses. I did not claim that relative deprivation theory provides the correct explanation. Nor did I claim that the J-curve theory may not be considered to constitute one form of relative deprivation theory. Rather, I asserted that the data they presented did not lead logically to a refutation of relative deprivation theory. Their claim to the contrary appeared to spring from a lack of familiarity with the relative deprivation literature and from a looseness of operationalization.

My position is, I think, supported by the Miller-Bolce communication. In several instances (e.g., paragraphs 4, 11, 14, 16, and 19, pp. 818-20), Miller and Bolce announce that, in their 1977 article, they were concerned only with Davies' J-curve theory. Obviously, the J-curve is one formulation of relative deprivation theory. Because they were concerned with only one version of the theory, they were prevented, logically, from drawing conclusions...
about the theory as a whole.

Specific Issues

The Miller-Bolce letter raises numerous issues. I would like to discuss nine of them, taking them in the order of appearance in the Miller-Bolce communication.

1. Miller and Bolce state in their opening paragraph that one of my "primary objections" to their article was their failure to define relative deprivation. In fact, the definitional failure was but one example of what I took to be the broader problem: their misrepresentation of the literature. Other aspects of the problem in my view included their equating J-curve and relative deprivation theory, their ignorance of the work of Davis (1959), Runciman (1966), and myself (Crosby, 1976), their slanted view of Gurr (1970), and their false claims about the empirical research performed in the area of relative deprivation.

2. The communication accuses me of claiming that "Davies' theory is not a form of relative deprivation theory" (p. 818). In fact, I said that it is one form of relative deprivation theory. My claim was simply that Miller et al. had mistaken a part for the whole. In my article, I stated it this way: "Arguing that J-curve theory is invalid is one matter. It is quite another matter to argue that because J-curve theory is invalid, all of relative deprivation theory is invalid." (It is, incidentally, my belief that Miller and Bolce fail to demonstrate that the J-curve is invalid. Davies [1978] points out some of the ways in which Miller et al. present a "deficient" test of the J-curve hypothesis.)

3. The tenth paragraph opens with: "Crosby parts company with Davies. . ." (p. 819). It seems that Miller and Bolce are referring here to some parts of an unpublished manuscript which I sent them (Crosby and Bernstein, 1978). I am confused, however, by the reference to Davies. Bernstein and I discussed Davis, not Davies.

4. Miller and Bolce suggest that the only problem I found with their view was that it did not agree with my own. Among the sentences one could cite are these: "The crux of Crosby's quarrel with us is really that her understanding of relative deprivation differs from ours. . ." (p. 819), and, "Her quarrel is based on our not having set out to do what she has done" (p. 819). It seems ironic indeed that Miller and Bolce give me and my formulation of relative deprivation proportionately much more attention than I myself did.

My original piece did not claim that Miller, Bolce, and Halligan ought to know my model of relative deprivation. On the contrary, I explicitly excused them for not knowing it. I did chide them, however, for not knowing the work of Runciman and of Davis. I remain dismayed by their continued silence about these scholars. Runciman and Davis deserve more than the passing glance they receive in the Miller-Bolce communication. On p. 819, Miller and Bolce characterize my concern for Runciman and for Davis, and (by implication) for Gurr as "irrelevant" and ask if they should be expected to test all the relative deprivation theorists. The answer is obvious. If one wants to make claims about relative deprivation theory in general (as they did in 1977), then one must give the theory a fair hearing. No part can stand for the whole.

5. Miller and Bolce continue to assert that relative deprivation theory assumes a correspondence between present conditions and future expectations. To support their claim that Gurr sees the present-future link as "fundamental to the application of relative deprivation theory to the study of collective violence" (p. 820), they refer to Gurr's ongoing work with Duvall. Concerning the Gurr-Duvall collaboration, the 1977 article read: "All we can say is that sometimes, and for some groups, future expectations are a function of current need fulfillment, and at other times, for other groups, this relationship does not hold" (Miller et al., 1977). Which tune are we to follow?

6. On the issue of "felt deprivation" Miller and Bolce seem off the mark. If they accorded Davis and Runciman the attention they deserve as relative deprivation theorists of long standing, Miller and Bolce would have to admit that deprivation is conceived as a feeling. Even Gurr speaks of a tension state. Davies too mentions emotions. One distinction that Miller and Bolce make is rather too subtle for me. They show no interest in "whether blacks 'felt' deprived" and restrict their curiosity to "whether the black population experienced . . . a pattern of relative deprivation" (p. 820). I do not know how one can experience a pattern. Nor do I follow how one can separate feelings from experiences.

7. In my article I suggested that Miller et al. ought not to have drawn conclusions based on a comparison of their Table 1 and their Table 2. We see from their communication that the former contained aggregated census data; the latter contained individual-level survey data. Whether the census data are superior or inferior to the Michigan survey data is irrelevant. The point is that they are different. This means that the observed differences between Tables 1 and
2 could have been owing to (a) the different levels of data (aggregate-individual) as Miller et al. claim or (b) something about the way in which the data were collected. The alternative is plausible.

8. Miller and Bolce believe that I made a mistake in my own small statistical analysis. They claim that one may not test the nature of a curve by analysis of the individual points. At least one standard statistical textbook (Hays, 1963) suggests the opposite; Miller and Bolce thought I had tested to see if the "curve is other than would be expected by chance" (p. 821). I did not test to see if the curve for northern blacks differed from chance variations. I did test to see if the variance in that curve differed significantly from the variance in the curve for the northern whites. Their own original uncertainty argument rested on their "eyeball" observation that the northern black curve fluctuated more than the other curves. I was attempting to provide a more precise test of their speculations than they themselves did.

Since writing my original piece, I have learned of an analysis which might be equally or even more appropriate for the issue of fluctuations than is the test for heterogeneity of variance. It is Tukey's test for smoothing curves (Tukey, 1977, esp. chapter 7). To use this test, one would have to convert the aggregate data into single points, as I did in the test for heterogeneity of variance. The advantage of Tukey's test is that it is sensitive to dips and ridges, while the standard test for heterogeneity of variance is not.

9. I am interested by the dissertation data analysis presented by Miller and Bolce at the end of their letter. They claim the operationalization of relative deprivation, described on p. 821, "incorporates the notions suggested by Crosby as well as group and individual frames of reference" (p. 821). As far as I can tell, the measure does get at two of the hypothesized preconditions of my model (Crosby, 1976). But what has all this to do with assessing Davies' (1962, 1969) rise and fall model with which it is linked in the text?

Musings

Let me reiterate a point. Neither in my original article nor here do I wish to claim that relative deprivation theory provides the best explanation of black urban unrest. My central point is simply that the data and the analyses in the Miller, Bolce, and Halligan article and in the Miller-Bolce communication fail to refute relative deprivation. Why, then, all this fuss?1

One reason for the fuss, I think, has to do with the nature of social scientific inquiry. What differentiates scientific and logical endeavors from other endeavors is a matter of method and not of conclusion. One may conclude in the next years or months that relative deprivation theory is wrong or that it is right. Quite apart from the conclusion itself is the method one uses to reach the conclusion. The scientific method involves precision and logic. In the end, then, my concern with the work of Miller et al. rests on my conviction that in the social sciences we ought to care about how we reach our conclusions as much as we care about the conclusions themselves.

FAYE CROSBY

Yale University

References


1That my own fussing has been tempered considerably is due in large measure to Robert Abelson, Travis Crosby, and especially Donald Kinder. I am grateful for their help.
Comment by Davies

TO THE EDITOR:

As the accused and admitted parent of a theory that helps explain political violence, I want to say something about how the idea was conceived, its gestation, and about its continued growth since it was born in 1962. The idea, in its original or improved versions (Davies, 1962, 1969), has been published in whole or in part more than 25 times, but the occasional abuse and misunderstanding of this brainchild compels a parental defense. An earlier analysis in this Review (Davies, 1978) of the critiques by Miller, Bolce, and Halligan (1977, 1978) of the J-curve seems not to have established clear contact with the critics. At least, they have not very explicitly responded to my queries about what they think black people wanted in their 1960s rebellion and about when cycles begin.

The Ancestry and the Gestation

The J-curve's ancestry is a bit complicated but clearly traceable, at least back to the second generation. The idea was conceived about 1956, when I was trying to figure out why the 1894 strike of Pullman railway car workers took place when it did. Census Bureau data on both wages and prices for the three decades following the 1861–65 Civil War revealed a steady, slow rise in real wages among working people. Wages remained roughly constant; prices declined. During the 1894 recession, the gradual, generation-long rise in real wages was interrupted abruptly, most particularly for the Pullman railway car workers. Many people who worked in the Pullman shops and lived in the town which the company had built to house them were put on part-time schedules or were laid off altogether. Many of these people had very little to eat and were simultaneously pressed by the company to pay their suddenly delinquent rents. A nice J-curve describing the generation-long event popped out of the data, into my head, and onto the page of notes I was making.1

1The data on real wages are in U.S. Bureau of the Census (1949, pp. 66, 231–32, 235). See also Lindsey (1942, esp. Chs. 5 and 7).

The idea grew and was nourished by the writings of some rather remarkable people, whom I charge with grandpaternity: Crane Brinton's Anatomy of Revolution, which I had read when it was first published in 1938; the work of Abraham Maslow, whose need hierarchy (1943) is a basic component of the theory and the research of now several political psychologists (Davies, 1963; Aronoff, 1967; Knutson, 1972; Renshon, 1974; Inglehart, 1977; and Burns, 1978). And there were two remarkable others, whose most relevant work I encountered after first plotting the J-curve.

Tocqueville’s 1856 analysis of what led to the French Revolution noted in grand detail how things in France steadily improved during the eighteenth century, not just culturally but also economically and socially. (From Brinton and others I’d already got information about the 1788 economic crisis, which included a critically sharp drop in the supply of grain.) To this was added rumination about Marx and Engels’ 1848 Communist Manifesto which hammered on the ways industrialization was making the life of factory workers steadily worse. In the spring of 1960 Seymour Martin Lipset pointed out to me that Marx had written not only about increased immiseration but also about how people compare their circumstances with those of people in other classes: “Our desires and pleasures spring from society . . . Because they are of a social nature, they are of a relative nature.”

Three major components of theorizing about revolution were thus related to the writings of Tocqueville and Marx and Engels—jointly and separately. Tocqueville described an upswing before revolutions, Marx and Engels a downswing. And Marx (1849) clearly stated, in one of its forms, what Stouffer (1949), Pettigrew (1967), and Gurr (1968, 1969 and 1970), long after, called relative deprivation.

Stated most simply, the theory that was conceived about 1956 in a study of the Pullman Strike, nourished in utero mentis by the ideas of Brinton, Maslow, Tocqueville, Marx, and Engels, and born in 1962, states this: when a relatively long period of steady rise in what people want and what people get is followed by a short period of sharp reversal, during which a gap suddenly widens between what people want and what they get, the likelihood of revolution increases sharply.

The Growth of the Idea Since 1962

The Black Rebellion and the Student Rebellion of the 1960s rekindled interest among