Costs and Benefits of Defying the Crowd in Science

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Scientists, mirroring the societies in which they live, have devised numerous ways of rewarding conformity and punishing defiance. Some of the mechanisms are reviewed. Scientists who defy the crowd can gain extrinsic reinforcement, but often from sources that promote irresponsibility on the part of these scientists. For the most part, Arthur Jensen has spent his career in defiance of the scientific crowd. Some of this work has made an outstanding contribution to the science of intelligence; other work, I believe, has been regressive. What kind of system might appropriately reward that work which has made a contribution?

On November 27, 1997, the day of the Macy's Thanksgiving Day parade, my wife and I were in New York City. We had no interest in the Thanksgiving Day parade and were walking down Seventh Avenue when we started encountering noticeable pedestrian traffic walking in the opposed (northward) direction. The farther we walked, the heavier the opposing pedestrian traffic became, and the more visibly annoyed people became that we were walking in the opposite direction. Eventually, we reached a density of human traffic such as we had never seen before. The choice of whether to walk in the direction opposed to everyone else was taken away from us: Police waved us over onto a side street to head toward the east. We could either join the crowd or leave it altogether. We were forbidden to oppose it.

Throughout much of history and much of the world even today, people have had the same choice with respect to their ideas. They have had the option either to join the crowd or, if they are lucky, to leave it, but not to oppose it. Through secret police, inquisitions, kangaroo courts, and even summary execution, people who have chosen either not to be part of the parade or at least to be fellow-travelers with it have been subjected to punishment. Of course, people in the United States like to believe that it is different there. After all, many of those living in the U.S. believe it to be a "free country."

On the one hand, the freedoms enjoyed by residents of the U.S. are substantial. For example, people can criticize the government or even sue its chief executive, both without

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being imprisoned or otherwise totally silenced. At the same time, the society has been able to function without a formal government-sponsored "thought police" in part because the members of the society have themselves taken on aspects of the role of a thought police, obviating the need for a formal squadron. The society has devised many ways to punish nonconformers, as any child in a schoolyard has observed. Of course, at other times in the country's history—most notoriously but not only during the McCarthy era—nonconformers in the U.S. have not been so lucky. Those who have defied the crowd have been vilified or even perished. Scientists are not much different from other people. Scientists, too, have developed a number of ways to ensure that their numbers follow the crowd.

Enforcing Conformity to the Crowd among Scientists

Thought policing is not limited to politics. It occurs in science as well. Many individuals enter science because they believe it is a calling that encourages free thinking and independent thought. Many of these same individuals soon discover that their idealism bears little contact with reality. As Kuhn (1970) and others have observed, scientists are no more independent-minded or free-thinking than anyone else. If anything, they cherish conformity more than the rest.

Scientists enforce conformity in a number of ways, both formal and informal.

1. Training. For the most part, students learn through their preprofessional training what the current paradigms are and what kinds of work are rewarded and what kinds are not. They are encouraged to do kinds of work that will be rewarded. To a large extent, training is considered "good" to the extent that it teaches students where the rewards are.

2. Publications. Many people who have submitted articles to journals have discovered that the refereeing process is an excellent way to ensure conformity under the banner of quality control. Of course, it is difficult to get articles accepted if they are totally pedestrian; but there is almost always some journal that will take an article, no matter how pedestrian it may be. More difficult is to get articles accepted if they go against the accepted wisdom, as John Garcia discovered in his studies of conditioning and as many others have discovered in their own work. Thus, many people find that the work that is hardest to get accepted is not only their worst work, but also their best.

3. Grants. Grants provide an excellent way to reward conformity. People who work outside established paradigms often find it very difficult or impossible to get funded, so that they are effectively prevented from doing much of what they might have intended to get done. There are many forces that contribute to making granting agencies a conservative force (see Sternberg, 1996a, 1997). First, low selection ratios allow even one negative reviewer essentially to blackball a proposal. Second, programmatic agencies fund work within their established program of research but not outside it. Third, people who are asked to serve on review panels will, for the most part, be those working within established and accepted paradigms. Fourth, those who agree to spend the vast amounts of time it requires to be on such panels may tend even more toward conformity than those who would rather devote the time to their own research. Finally, proposals are expected to make contact with existing paradigms, and if they do not, they can be rejected for this reason alone.

4. Recognitions. Through prizes, awards, organizational offices, and the like, scientists can enforce their set of values, recognizing those who play the accepted game well and failing to recognize those who go outside the accepted limits. In some cases, these views may even have nothing to do with the work for which recognition is being given. For example, a lifetime achievement award to be presented to Raymond Cattell at the annual meeting of the American Psychological Association in 1997 was suspended pending investigation of his religious beliefs!

5. Book Reviews. Books of scholars who go beyond the limits are typically subject to negative reviews, sometimes by people who seem not to have read the books.

6. Graduate Students. "Respectable" researchers do not send their undergraduate students to work with "disreputable" researchers.

7. Informal Networks. Perhaps most importantly, those who work outside accepted networks never make it into the professional in-groups. They are less likely to be asked to serve on committees, write promotion letters, give invited talks at major scientific conferences, give departmental colloquia, and the like.

In sum, the various fields of science construct a system for enforcing conformity. Scientists who do not conform are "out." But other mechanisms come into play that not only reward scientists for divergent views, but actually encourage the scientists to diverge even more, even to the point of irresponsibility.

Encouraging Nonconformity to the Crowd among Scientists

If scientists received no rewards at all for nonconformity, they might cease to be nonconformists. But there are at least three major sources of rewards for nonconformists.

1. Internal Rewards. Scientists who state what they believe and then fight for their beliefs have the satisfaction of knowing that they are saying and doing what they believe in. They can also hope that, in the long term, the scientific establishment will come around to their way of thinking and reward what they are doing. In fact, such changes are not unusual. In their writings, both Sandra Scarr and Robert Plomin have commented on how behavior-genetic work that was devalued in the 1970s came to be valued by the latter half of the 1990s.

2. Fringe Groups. Fringe groups of scientists may set up their own organizations to reward what they are doing, or may find that their work is ideologically consistent with the priorities of political or social fringe groups and thus accepted and even welcomed by such groups. Such scientists may therefore find themselves having to decide whether to associate with these groups in order to feel extrinsically rewarded. But these groups may in turn encourage the scientists to take positions even more extreme than those they believe in, and perhaps to take positions that are irresponsible.

3. The Media. By far the most powerful ally of the nonconforming scientist in this country can end up being the media. The media thrive on controversy and on the offbeat. Thus if virtually all scientists believe that AIDS is caused by the human immunodeficiency

virus (HIV) and a few scientists do not believe this to be the case, the disbelievers may find themselves actually getting more media attention because of the divergence of their views. If most people believe that racial differences in psychometrically measured intelligence are largely environmental and a few scientists believe (or are willing publicly to state) that such differences are probably largely genetic, the views of the minority are likely to attract attention. Moreover, even if most of these scientists' views are conventional, it is the unconventional part of their views that is likely to attract the media attention.

The situation can become pernicious because media attention tends to be short-lived. Unfortunately, almost the only way for nonmainstream scientists to maintain media attention and the extrinsic reinforcement it brings is either to take new unconventional positions, or to become more extreme in the positions they already have taken. Many of us scientists who have worked with the media have found reporters trying to get us to make statements more extreme than we really believe, simply because such statements make for more interesting press coverage. The reinforcement system thus can turn a nonconforming but responsible scientist into a less responsible or even an irresponsible one. Worse, it may be only through the media that one can gain any coverage of one's divergent views. Iced out of mainstream science, scientists with nonconforming views may thus turn to the media to get press coverage of their views, not fully realizing the dangerous game into which they are entering. Of course, the press coverage further "turns off" the so-called respectable scientists, so that what formerly might have been a bad situation with regard to the scientist's participation in mainstream science becomes an even worse one.

The Role of Defiance of the Crowd in Science

Defiance of the crowd in a Thanksgiving parade is rather innocuous. In science, defiance of the crowd has higher stakes. Elsewhere, Todd Lubart and I have proposed that defiance of the crowd is the hallmark of creativity (Sternberg & Lubart, 1991, 1995). Individuals in science or any other field who make the most difference are those who defy the crowd. These individuals generate ideas that, like stocks with low price-earnings (P-E) ratios, seem unattractive and even repugnant to others. The individuals work to raise the value of their metaphorical stocks, attempting to convince other people of the value of their ideas. Ultimately, they metaphorically "sell high," moving on to their next unpopular idea.

In our work, we give numerous examples of how initial receptions to creative ideas are often unfavorable and even patently hostile (Sternberg & Lubart, 1995). Scientists have developed a number of ways to ensure that scientists follow the crowd.

What's the problem, then? Why not just institute some kind of guarantee that scientists who defy the crowd will be rewarded rather than punished? The problem stems from the fact that creativity is typically defined not only in terms of novelty, but also in terms of quality and appropriateness. In terms of the stock-market analogy, one needs to remember that many and probably most low P-E stocks never do rise much in value. Consider the example of HIV and AIDS.

The scientist who denies that the human immunodeficiency virus causes AIDS takes a large risk. He will be disparaged by other scientists for defying the crowd. But if he can show to their satisfaction or that of others who hold power in the society that he is correct, then he may actually end up being a hero. In the case of the HIV opposition, no such demonstration has emerged. Nor has any credible science emerged from the efforts of propo-

nents of cold fusion. In both cases, novelty without perceived quality has led proponents of offbeat views to be labeled not as creative, but rather, as crackpots. Creative people, of course, are risk-takers, but they tend to be sensible risk-takers (Sternberg & Lubart, 1995). They are willing to take risks that, in the long run, are more likely to pay off. The risks taken by proponents of the theory that HIV does not cause AIDS and of the theory of cold fusion have, to date at least, failed.

THE CASE OF ARTHUR JENSEN

Where does Arthur Jensen fit into the schema that has been set up in this article?

First, most of Jensen's career has been outside mainstream science. Since his article almost 30 years ago in the *Harvard Educational Review* (Jensen, 1969), Jensen has been viewed by many as outside mainstream science. This fact is ironic, because the overwhelming majority of his articles and books have been within mainstream science. Jensen's work on reaction time and intelligence (e.g., Jensen, 1982) is solidly within the information-processing tradition. Jensen's work on test bias (Jensen, 1980) is solidly within the psychometric tradition, as is his work on the *g* factor (Jensen, 1998). Thus, what constitutes a relatively small proportion of his work has, for the majority of the scientific community, defined him. As noted in an invitation letter to this symposium that "he has received very little official recognition for his work and probably will not in the future" (Detterman, 1997), Jensen's defiance of the scientific crowd has cost him. Awards and recognitions that he otherwise might have received for influential, highly cited work may never come.

Second, Jensen has been courted both by political fringe groups and by the press. Not despite but rather because of the unpopularity of his views, Jensen has been a media figure to an extent that is rare in mainstream psychology. Few psychologists are as well known, and some who perhaps are, such as the late Richard Herrnstein, are known for much the same reason—not for their mainstream work (in Herrnstein's case, on animal learning), but for their work on race, heritability, and intelligence.

Scientifically, I disagree with most of the corpus of Jensen's work for reasons that are not relevant to this article but are discussed elsewhere (e.g., Sternberg, 1985, 1996b). But in terms of the criteria by which I believe scientific work should be judged—such as creativity, basis in theory, empirical rigor, and impact—I believe that most of Jensen's work fares well. The corpus of Jensen's psychometric work on the general factor and of his information-processing work on reaction time—but not of his behavior genetic work, including work on racial differences—place him as one of the outstanding leaders in the field of human intelligence. Indeed, few people now alive have had more impact on the field, for better or worse. And few people studying human intelligence have more scientific investigations to their credit. Indeed, much of the highly cited work in the field of intelligence has little or, arguably, no scientific basis at all.

I exclude from this accolade Jensen's work on behavior genetics and racial differences in intelligence because, for a number of reasons discussed elsewhere (e.g., Sternberg, 1996b), I believe this work to be not only wrong, but wrong-headed. My goal here, though, is not to discuss substantive differences, but rather, how a field should evaluate scientific work that defies the crowd.

MODIFYING THE SCIENTIFIC REWARD SYSTEM

Arthur Jensen is, in my opinion, an epitome of the need to change the reward system in science. Suppose, for the sake of argument, that many other scientists believed as I do that Jensen's work on information processing and on psychometrics has been ground-breaking but his work on behavior genetics has not been, or even has been regressive. How does the reward system function?

Much of the way the academic reward system functions—not just in science—is by the reputation of the academic (Caplow & McGee, 1958). When reputation is viewed unidimensionally or almost unidimensionally, the field may find itself forced into judgments it should not make. If one body of work within a corpus is disfavored, scientists may end up generalizing this disfavor unfairly to other work by the same investigator.

At times, some kind of combination formula with regard to the bases of evaluation is inevitable. For example, when a department has just one available slot for a job and someone must be hired, a hard choice must be made despite the fact that an idiographic model of evaluating candidates might seem much more appropriate than a nomothetic model. But many decisions need not be unidimensional.

Many and probably most major scientific awards are to individuals for the cumulative corpus of their work. As a result, a scientist who has done even one stream of unpopular work may find him or herself iced out of the awards system because this work damages—rightfully or wrongfully—the valuation given to the overall corpus or work. Perhaps a better way to grant recognition would be to a program of work, with the individual rather than the work being seen as incidental. Thus, instead of giving an award to Scientist X for Research Program A, the award would be given to Research Program A—not necessarily the whole corpus of a scientist's work—with the scientist receiving the award incidentally. The focus would be on the work, not on the scientist. In the case of Jensen, one could recognize the value of his work on reaction time or the general factor without recognizing the value of other work. In the case of Cattell, one would reward the work, say, on the theory of fluid and crystallized intelligence or on the 16-personality factor theory irrespective of what Cattell's personal beliefs might be.

In some cases, judgments of work are being influenced not by portions of the person's work, but by judgments of the person's character with respect to things that arguably have nothing to do with the work. Cattell's religion is a case in point. A more extreme example is Paul DeMan. The work of Paul DeMan, in particular, and deconstructionism in general, are undergoing a thorough reexamination in light of fairly recent discoveries that DeMan wrote virulently anti-Semitic tracts in his youth. Such tracts certainly may and probably must greatly damage our evaluation of DeMan as a person. But Richard Wagner the composer and Ezra Pound the poet were also virulent anti-Semites. Their work stands as it was, regardless of how personally despicable either or both of them likely may have been. It would probably be a loss to the world if Wagner's and Pound's works were ignored because of their despicable personal views or because of their deeply flawed personal characteristics.

If we are to believe Gardner (1993), many creative individuals have had much less than savory personal characteristics. There is good reason to judge people and their work separately, and then to judge people's distinct programs of work separately. Indeed, almost

every creative individual has produced work of which he or she is, at best, not proud, and at worst, ashamed (or should be).

Conclusion

Science has a number of ways of enforcing adherence to the dictates of the crowd. Scientists who choose to defy the crowd can still gain reinforcement, but when it comes from others, it is often in the form of temptations that can lead the scientist down a path to irresponsibility. Scientists would do better if they focused their evaluations not on individuals, but on programs of work within the total corpus of the scientists' work. In this way, people whose work is viewed as undesirable in some ways are not punished so that neither they nor other work they may do is taken seriously.

In the case of Arthur Jensen, I believe that a large body of his work is deserving of great commendation (although I disagree with most of it). I hope it is for his work on information processing and the general factor that he is remembered, not for his work on behavior genetics, test bias, or racial differences in intelligence and related traits.

If there is anything for which citizens of a country should give thanks on Thanksgiving Day, it is not that they can join a parade, but that they can choose to walk, at the very least, away from it, and at best, in opposition to it.

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