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NORBERT L. KERR, Michigan State University

HARK! A Herald Sings...But Who's Listening?



*A reader quick, keen, and leery
Did wonder, ponder, and query
When results clean and tight
Fit predictions just right
If the data preceded the theory.*

This little limerick, authored by a publicity-shy colleague, appears as a preface (and, I suppose, a summary) of what feels to me like my most underappreciated paper (Kerr, 1998). Such feelings, of course, are determined by two independent sources—objective signs of appreciation (e.g., where the work is published, how often it is cited, is it widely discussed, does it make it to textbooks, etc.) and one's subjective hopes for how the work will be received. The larger the gap between hoped-for appreciation and actual appreciation, the more underappreciated one sees the work (and the more disappointed one is). To harken back to this particular project, I now wonder whether my disappointment stems more from my unrealistic hopes than from any real lack of appreciation of my work. So my story, perhaps like many in this volume, may reveal as much about me as it does about the work.

A LITTLE EARLY HISTORY

It is often hard to pin down exactly where ideas come from. But I think this work had two origins—in some teaching and in some editorial work. I occasionally teach a graduate research methods course, and on one occasion I used Judd et al.'s (1991) excellent textbook. I especially liked the fact that the text pays some serious attention to topics that often get short shrift in social psychological training—for example, non-experimental methods, data coding, how to write up one's work. The latter topic is the special focus of Chapter 19, by Daryl Bem (1991), a revision of an earlier chapter (Bem, 1987). Both chapters are full of very good advice on how to write a clear, engaging, and publishable research report. But there was one piece of advice that bothered me:

There are two possible articles you can write: (1) the article you planned to write when you designed your study or (2) the article that makes the most sense now that you have seen the results. They are rarely the same, and the correct answer is (2) . . . the best journal articles are informed by the actual empirical findings from the opening sentence. (Bem, 1987; p. 172)

This text seemed to legitimize taking a genuinely *post hoc* hypothesis, one not foreseen or credited when the study was conceived and designed, and presenting it in the introduction of one's write-up as if it were a genuine *a priori* hypothesis, one that justified and guided the research. Eventually I came to call this practice HARKing (an acronym for *Hypothesizing After the Results are Known*). In fairness, Bem does not quite advocate HARKing. However, as I discussed his advice with colleagues, I found that several mentioned Bem's chapter as a justification for HARKing.

Why did this bother me so much? Initially, it was simply what looked to me like HARKing's patent dishonesty. Surely it couldn't be ethical to rewrite the true history of one's research so that one's final best understanding masqueraded as one's original understanding! However, I couldn't find any prohibition (or even mention) of HARKing in ethical guidebooks (e.g., the *APA Manual*). So, for a while, I simply contented myself with grouching about HARKing when we discussed report writing in my classes.

The other impetus for doing more than such grouching came from my experience as an associate editor at *JPSP-IRGP* in the late 1980s. While doing the editorial work, I noticed several things:

1. Authors who weren't prescient—those who presented hypotheses in the introductions to their papers that failed to be confirmed—rarely got positive reviews.
2. Non-prescient authors who presented quite reasonable *post hoc* interpretations of their findings routinely got one (sometimes both) of two types of reviews: (a) this paper is not publishable unless/until the author does another study

to independently confirm his/her *post hoc* hypothesis or (b) a revision of the paper in which the author HARKs—viz., replaces the introduction's original, inadequate hypotheses with the author's *post hoc* hypothesis—might well be publishable.

3. I kept seeing circumstantial evidence of HARKing, including (a) theoretical assumptions that didn't seem connected to the paper's central theoretical argument but that (so conveniently) helped explain some twist of the data and (b) experimental designs that failed to include conditions or measures that—given the ability of the author—one would have expected if an ostensible *a priori* hypothesis actually did guide the design of the experiment.

Informally discussing this with colleagues and brother/sister editors suggested that I wasn't imagining things—many others had (informally) noticed similar patterns.

EXPLORING HARKING

Gradually I moved from fretting and grouching about HARKing to studying it in somewhat more systematic fashions. I did a survey of social scientists from three disciplines to get some descriptive data (e.g., Does HARKing occur? How much? Why?) and some prescriptive information (i.e., Should HARKing occur?). The survey suggested that HARKing was widespread but that (at least some) forms of HARKing were widely seen as improper. That got me even more interested—surely it would be important to examine a practice where we might be “preaching one thing and practicing another.” I started poking around for relevant prior work. My experience has been that the better the research question, the more interesting and relevant stuff you can find, but only if you look; confirming that many smart and thoughtful people have wrestled with the same problem can reassure you that you're working on a really good (i.e., complex, challenging, important) problem. For HARKing, I found lots of interesting and relevant stuff, scattered all over the place (in philosophy [e.g., Horwich, 1982], in cognitive psychology [e.g., Simon, 1955], in ethics, in discussions of good storytelling and writing, in statistics, in literature [e.g., Sherlock Holmes had strong opinions on HARKing]). This fed my conviction that understanding the roots and effects of HARKing was a really good problem. So did the reactions I got in my initial attempts to present my ideas to my colleagues in the early 1990s. The reactions were usually strong, ranging from outrage that I was airing “dirty linen” in a way that might damage the discipline of social psychology to outrage that anybody might ever engage in HARKing to relief that finally somebody was talking about a dilemma that authors constantly face. I seemed to be hitting a nerve.

All of this came to a head when I took a sabbatical in the spring of 1995 at Leiden University. I went alone (I was in the midst of a painful divorce [is there any other kind?]) with a head full of jumbled ideas, a suitcase full of notes, the outline of my HARKing lecture, and a vague intent to finally pull something coherent together.

Although it was a miserable time personally (e.g., intermittent long-distance calls to my four-year-old son), it was professionally an ideal sabbatical—with smart and generous colleagues (at Leiden and the many other places I peddled my traveling HARKing show), time to think and write, and an interesting problem to work on. I came back with a first draft of what would eventually become the Kerr (1998) paper.

I won't try here to summarize fully the content of the paper. Basically, I tried to clarify what might constitute an act of HARKing, summarize the results of my survey research, identify some of the forces (both psychological and professional) that encourage HARKing, identify what some of the costs of HARKing might be for science, and (reflecting my personal conviction that the costs exceed any benefits) I proposed a few ways that HARKing might be deterred. The paper was considerably improved by the encouragement and suggestions of the action editor at *PSPR*, Marilynn Brewer, as well as of many others (Pat Laughlin's comments were especially helpful).

APPRECIATED?

As I suggested earlier, by most of the objective and usual criteria, this paper is a poor candidate for being “most underappreciated.” It was published in an excellent journal. It has been cited respectably (including in a couple of subsequently published research methods textbooks, the origin of my interest). A few colleagues have told me that they now require or recommend that their own research methods students read the paper. Most gratifying were the comments of several well-respected colleagues who said very nice things to me about the paper. Still, I remain deeply disappointed in the paper's (lack of) impact. Its last sentence reads, “If this article helps encourage discussion, debate, and research on HARKing, it will have fulfilled its purpose” (p. 216). But if there has been lively discussion and debate on this issue in (or outside of) social psychology since the paper's appearance, it has escaped my notice. And, as best I can tell from my perspectives as a reader, reviewer, and editor, the symptoms of HARKing in published social psychology are at least as commonplace today as when I first began my grouching.¹

Now, it could well be that HARKing is really not a significant problem; I may have merely convinced myself that it is. More bothersome is the possibility that HARKing really is a serious problem—one worth discussing, debating, and researching—but that I simply failed to convince others of this. Maybe other chapters in this volume will echo this theme—the biggest disappointments are felt when you have tried your best to excite others about an idea or a problem that has really excited you, but failed.

So, the roots of disappointment about reactions to one's work lie at least as much in one's own excitement about the work, one's own expectations about its likely impact, as in others' actual reactions. Maybe the HARKing piece is a source of

particular disappointment to me because it did not merely address a substantive social psychological problem that only a small handful of scholars also care about. (I suppose I could have nominated any of several papers of mine focused on jury behavior, social dilemmas, or group decision making as my “most underappreciated” work, although this would be a rather difficult, “Sophie’s Choice” undertaking.) But the HARKing paper was not just targeted at a few fellow specialists; it touched on a problem that most scientists must confront when they sit down to write up their work. We tend to get more excited by our ideas when we think that many others will (or at least should) be similarly excited. When they aren’t, it can be quite a letdown.

The reward structure in academic life is often characterized as a “lean reinforcement schedule,” meaning that one has to work very long and very hard to get one’s rewards. Those rewards are sometimes explicit and tangible (like recognition, interacting with interesting people, job security) and sometimes implicit and intangible (like getting to choose one’s problems and scratching the itch of curiosity). While these rewards are real, they also tend to be uncertain, intermittent, and delayed. This may be why some people enjoy their teaching as much as their research, because the rewards can be more frequent and predictable: they can be garnered every Tuesday and Thursday.

AFTERTHOUGHT

In retrospect, it was probably a bit naïve (as well as delusional) to think that a single paper would ignite a firestorm of debate and controversy. This is particularly true for a practice like HARKing, which appears to be familiar, well entrenched, bolstered both by unconscious judgmental biases (e.g., confirmation bias; hindsight bias) and explicit expert advice (e.g., Bem, 1987, 1991), professionally functional, with few influential critics in high places (e.g., editors, textbook authors). As Thomas Kuhn (1996), among others, has pointed out, rapid change in scientific practice is the exception, not the rule. A better, more hopeful metaphor for mounting a challenge to a standard practice (even a demonstrably bad practice) may be the act of planting a seed, not igniting a fire. Plants, like fires, have to be carefully tended to grow. It takes a bit of the sting out of the fate of Kerr (1998) if I think of my writing the chapter you are now reading as watering a struggling plant (and not as blowing on the embers of a dying fire). And to the next generation of social psychologists, I’d echo Jackson Browne’s advice:

*Into a dancer you have grown
From a seed somebody else has thrown
Go on ahead and throw some seeds of your own...*

NOTE

1. The appearance of Kerr (1998) did have some impact, although not quite what I had hoped for. From time to time, I've had colleagues interrupt presentations of their work to assure the audience (while looking fixedly at me) that they *really* did have some particular hypothesis in hand before doing their study. Or, colleagues have come to me to ask whether one or another instance of presenting or revising a hypothesis is or isn't HARKing. On all such occasions, I assure my colleagues that I have neither the wisdom nor desire to function as a HARKing policeman.

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