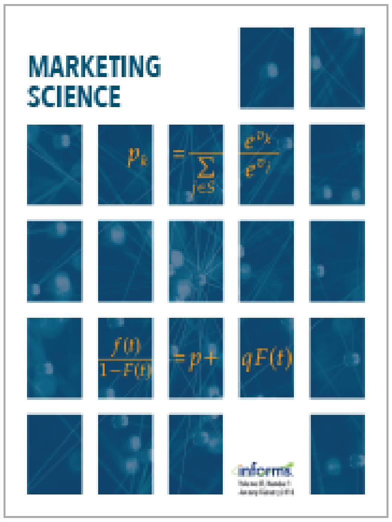

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Commentary

Mental Accounting and Consumer Choice: Anatomy of a Failure

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I am pleased, of course, that my paper on mental accounting was selected for this issue. The paper is one of my favorites, and the topic is one I continue to think and write about all these years later. In this note I would like to give a brief history of the paper and say why I consider it a failure in the sense that it did not achieve the goal I had in mind when I submitted it to *Marketing Science*.

Mental accounting is the process, sometimes implicit, by which individuals and households keep track of and evaluate their transactions. It serves very much the same function for households that financial accounting serves for organizations. The topic is one I first discussed in my earliest paper in behavioral economics (Thaler 1980), entitled "Toward a Positive Theory of Consumer Choice." In that paper I referred to the concept as "psychological accounting" but my friends, mentors, and later collaborators, Tversky and Kahneman (1981), suggested a better term "mental accounting," and I have adopted that term as well. My training is as an economist, not a psychologist, and my interest in this topic came from my inability to explain some behaviors, such as the failure to ignore sunk costs. It was obvious to me that theory and behavior were at odds here, but I could not pin down why. I felt that I could make some progress on this question by better understanding what happened, mentally, when purchases were made. Did people make mental debits and credits? If so, why would these mental accounting entries lead them to be more likely to eat a dessert if they paid for it, than if it were given to them for free? Since the paper itself follows this note, I will not summarize the answer, but for my latest thoughts on this question, see Shafir and Thaler (2006).

Since the paper is often cited, it seems odd that I would call it a failure. Why? Part of the answer lies in a comparison with another paper I published the same year (De Bondt and Thaler 1985). That paper is not cited quite as often as the mental accounting paper (454 times, according to the Web of Science), but it has been successful in precisely the way in which the mental accounting paper has failed. To explain the failure, I have to explain why I submitted the paper to *Marketing Science* in the first place.

By 1983, when I was getting ready to submit the mental accounting paper to a journal, I had spent about five years doing research that applied ideas from psychology to economics, a field that has since come to be called *behavioral economics*. This was a lonely activity to be pursuing at this time, and while I found the work interesting, I was interested in encouraging others to join the fun, so that I would at least have someone to talk to. It occurred to me that a natural place to apply some mixture of economics and psychology would be marketing. After all, I thought, much of what firms do in marketing their wares, from advertising to packaging, seems difficult to explain within the standard economic model. Why do beer companies, for example, spend so much money showing commercials with guys drinking beers, or gals in bikinis? Does this really provide information about beer? Since I was working in a business school I knew that marketing was also a field that already had both behavioral and quantitative types, so I thought (naively) that a blend would be a perfect match for marketing. With this in mind, I sent my paper off to Subrata Sen at the then-new journal, *Marketing Science*. Subrata and I had been colleagues briefly at the University of Rochester, and I knew he had some interest in this area. I had considered sending the paper to the *Journal of Consumer Research*, but I had been assured that there was absolutely no chance the paper would be accepted, a judgment that I think was probably right. Also, it was economists that I wanted to reach, not psychologists, so *Marketing Science* seemed like a better choice. Subrata sent the paper off to an associate editor and three referees. After a few months I got a rather sheepish letter from Subrata. Although Subrata liked the paper, no one else did. The Associate Editor was negative, bordering on hostile, and the Associate Editor and the referees all had lots of suggestions for how the paper should be changed, many of which were contradictory. Subrata wondered if I had any ideas on how to make this crowd happy.

My reaction was not helpful. After carefully reading all the reports, I told him that I had several ideas of ways to improve the paper, but none of the changes I had in mind were likely to make any of the readers happy. Furthermore, I felt that most of the changes that were suggested would make the paper worse. This was, of course, in the days before e-mail, which I think was probably for the best, because I would probably have sent at least one incendiary e-mail that would have sealed the deal. However, by phone Subrata was his usual amiable self and was able to act as peacemaker, doing some shuttle diplomacy between me and the associate editor, and the paper was eventually published. Over time, it has proven to be popular. So what about that failure?

My goal of generating a subfield legion of marketing scholars doing behavioral economics has been a complete bust. Although there are many psychologists in marketing interested in judgment and decision making, and even quite a few interested in mental accounting, the number of economists taking a behavioral approach to marketing research is somewhere between tiny and zero. To a first approximation, the field of behavioral economics-marketing does not exist.

Which brings me to the comparison with the work of De Bondt and Thaler (1985). That paper came to be written because I had a graduate student, Werner De Bondt, who was interested in finance, and we decided to see whether a simple application of Kahneman and Tversky's representativeness heuristic could be applied in financial markets. Specifically, we wondered whether the failure to understand regression to the mean that had been documented in psychology experiments could infest financial markets and actually influence prices. More specifically, we investigated whether stocks that had done extremely well or badly over the past few years would show mean reversion in the future. This idea was, of course, blasphemy. It was considered to be an article of faith, if not an indisputable fact, that it is not possible to predict future stock returns from past stock returns. In fact, several colleagues accused me of "advisor malpractice" in allowing Werner to work on such a dumb idea. Well, it turned out that our dumb idea worked, and that the reason why no one had ever found long-term mean reversion is that no one had ever looked (probably because they "knew" that it could not exist). Of course, De Bondt and I were lucky to stumble onto a new fact (and the role of luck in research is often discounted), but my point in relating this story is that behavioral finance, a field that also did not exist in 1985, is now a thriving enterprise. Bob Shiller and I have been organizing workshops twice a year at the National Bureau of Economic Research for years now, and we typically receive more than 30 submissions for each workshop. I do not mean to suggest that De Bondt and I are somehow responsible for this success. Rather, my point is that if you had asked me to bet, in 1985, on which field would be quicker to adopt behavioral economics methods, marketing or finance, I would have put all my money on marketing, and I could not have been more wrong. Why?

Given my abysmal ability at predicting this outcome, I may not be highly qualified to explain it ex post, but let me suggest two possible contributing factors. The first is that the methods De Bondt and I used in our paper (or that Shiller (1981) used in his even earlier paper) were completely standard empirical finance. Both papers rely on readily available standard data sets that can be used by any researcher. Anyone could reproduce, criticize, or extend our preliminary investigations (and many did). Thus, the entry barriers for young financial economists going into behavioral finance were quite low, and perhaps as a result many dipped their toes in the water, had fun, and decided to take a swim. In this view, behavioral finance came into being because it was, relatively speaking, easy.

My second explanatory factor is more speculative. I believe that one reason why behavioral economics has not taken off in marketing is that there are already lots of psychologists in the field. This is another thing I was naïve about in 1984. I thought that the existence of psychologists would make it easier to do behavioral economics in marketing, but I believe the opposite is the case. If Ph.D. students come into the market now that want to do a combination of psychology and economics, they are advised that this would be a career mistake because neither the quants nor the behavioralists will welcome this mixed training. Young scholars are encouraged to either be a psychologist, or an economist, but not both. This is probably good advice because to be successful, a behavioral economist in marketing will have to produce economics that the economists think is high quality and psychology that the psychologists think is up to snuff. (We in behavioral finance do not have psychologists refereeing our papers, thank goodness.)

Whatever the reason for this outcome, I think this is too bad, because marketing appears, at least from my perspective, to be two quite separate cultures, with (at least) half the audience bored during most departmental workshops. A behavioral economics approach could, in principle, be of interest to both sides. Or, everyone could agree to hate it, and go out for beers to celebrate their coalescence.

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