been honed and changed by years of training. Introspection can thus be overused to become a comfortable illusion for the theorist, and an obstacle for science. Experiments with oneself as subject may be easier in the decision sciences as implied by Roberts, but they are also of more limited value.

Author's Response

Self-experimentation: Friend or foe?

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Abstract: The topics discussed in this response are in four broad areas: (1) *Idea generation,* including the failure to discuss and teach idea generation and how to nurture new ideas (sect. R2), sources of ideas worth testing with self-experimentation (sect. R3), and unusual features of the situation that may have increased the discovery rate (sect. R4); (2) *Miscellaneous methodological issues,* such as the value of mental experiments (sect. R5) and the limitations of double-blind experiments (sect. R6); (3) *Subject-matter issues,* including the relationship of this work to other evolutionary psychology studies and a new way to test evolutionary explanations (sect. R7), as well as questions about the mood results (sect. R8) and the weight results (sect. R9); and (4) *Self-experimentation,* including its difficulty (sect. R10) and future (sect. R11).

R1. Introduction

Because self-experimentation is unfamiliar, commentators on the target article resemble early adopters, the first to consider buying new products, in their willingness to take it seriously. I am grateful for their thoughtful remarks.

R2. Missing traditions

The target article begins by noting the absence of data-gathering methods designed to generate ideas (see "Missing methods") but **Glenn** and **Cabanac** point out that this is part of a larger gap. Glenn saw in the target article "a sometimes subtle but pervasive message that our understanding of scientific method is limited by the grip of traditional formulations." One of those traditions is not discussing the origin of ideas. Glenn notes how, when she was a graduate student, discussions of scientific method began *after* the hypothesis had been formulated. This state of affairs has persisted; there is little discussion of idea generation in recent textbooks (e.g., Cozby 2004; Stangor 2004) and scientific articles that introduce a new idea rarely explain its source (Feynman 1972). Moreover, as Cabanac says, granting agencies have no place in their budgets for idea generation. Their attitude seems to be: *If it happens, fine, but don't expect us to pay for it.* This tradition, as Glenn says, creates missed opportunities – discoveries that could have been made but were not.

A closely related tradition is no discussion of how to nurture new ideas. If you have a new idea, what should you do? Test it, sure, but how? The target-article research made me vividly aware that I could not answer this question nor find anything written about it. Trial and error led to one answer: *Take baby steps,* that is, do the simplest, easiest test that could increase or decrease the plausibility of the new idea (Roberts 2001). In other words, raise the bar slowly. The larger the step – the larger the difference between what you plan to do and what you have already done – the more untested assumptions you are making. The more untested assumptions, the more likely one of them is wrong. This might seem obvious, but most research proposals I encounter involving new ideas do not abide by it. The result, as **Glenn** says, is a lot of wasted effort. A few years ago, I attended a research meeting at a leading school of public health. The eight or ten persons at the meeting (almost all professors or persons with Ph.D.s) were deciding how to do a study of asthma. They were about to begin a large and expensive experiment, involving about 100 families, to measure the effect of an apartment cleaning on the incidence of asthma and allergies. They had done a pilot study with two or three families; the next step was the study itself. I suggested that they first do a larger pilot study, pointing out that the existing pilot study had not tested several assumptions that the larger study was based on. No one agreed with this suggestion, and two of the persons with doctorates said they disagreed with it. The study went ahead with no additional pilot work and was a disaster. Recruitment of families turned out to be far more difficult than expected. Funding had come from the National Institutes of Health, supporting **Cabanac**'s doubts about the wisdom of granting agencies.

R3. Sour\ces of ideas to test

The target article might have been titled "Self-experimentation as a source of ideas for conventional research" or "Self-experimentation as a growth medium for new ideas," because, in each example, self-experimentation made a new idea more plausible. **Lubart & Mouchiroud** wonder where the nurtured ideas came from. "Being able to test ideas cheaply [via self-experimentation] allows one to entertain a wider set of ideas," they write. "But these ideas need to be generated."

How were these ideas generated? For each idea that led to or was supported by self-experimentation, Table R1 gives the precipitating event (what caused me to think of it) and states whether written records helped generate it. The line between *experiment* (done to see what happens) and *natural experiment* (a change that happens for other reasons) is blurry, because actions may have more than one goal. I ate less-processed food to lose weight (Example 1), but I also did it to find out if what I had been telling my students, based on research with rats, was true for humans. Table R1 calls it a natural experiment, because my main goal was to lose weight. Table R1 distinguishes between *short* experiments (lasting a few days or less) and *long* experiments (lasting longer), because a short experiment may be nothing more than trying something new.

Table R1 supports several commentators' views about how to generate ideas worth testing. **Schegloff** believes that "sustained and systematic observation" is under-appreciated. Indeed, Table R1 shows that written records were helpful in three or four cases. **Lubart & Mouchi-**

https://doi.org/10.1017/S0140525X04350065 Published online by Cambridge University Press

Table R1. *Sources of tested ideas*

Example	New idea	Precipitating event	Records helpful?
1	Eating less-processed food causes weight loss	Published research (Sclafani & Springer 1976)	No
1	Weight loss reduces sleep duration	Natural experiment (change of diet to lose weight)	Yes
1	Water-rich diet causes weight loss (*not confirmed*)	Listening (student's experience)	No
1	Breakfast affects early awakening	Short experiment (change of breakfast)	Yes
2	Watching TV has the same effect as human contact on circadian oscillator	Published research (Szalai 1972)	No
2	Morning TV influences mood	Short experiment (morning TV)	No
2	Evening faces influence mood	Natural experiment (dinner with friends)	No
2	East-west travel eliminates faces effect	Natural experiment (trip to Europe)	No
3	Standing causes weight loss (*not confirmed*)	Listening (many hours/day of walking associated with weight loss)	No
3	Standing improves sleep	Long experiment (standing more)	Probably
4	Morning light improves mood	Listening (friend's experience)	No
4	Morning light improves sleep	Short experiment (outside walks)	Yes
5	Better sleep can eliminate colds	Long experiment (more standing and morning light)	No
6	Water causes weight loss	Listening (weight loss on unusual diet)	No
7	Weight control theory	Published research (Ramirez 1990a)	No
7	Low-glycemic-index food causes weight loss	Prediction of theory	No
8	Low-glycemic-index pasta causes weight loss (*not confirmed*)	Published research (list of glycemic indices)	No
9	Sushi causes weight loss	Natural experiment (all-you-can-eat sushi meal)	No
10	Sugar water causes weight loss	Natural experiment (soft drinks in Paris)	No

Note. Experiment = Self-experiment. Short = Lasted a few days or less. Long = Lasted more than a few days. Records helpful? = Did written records of mine help inspire the idea? *Not confirmed* = Self-experiment made idea less plausible.

roud emphasize taking advantage of "natural experiments." Natural experiments led to five ideas. **Moore & Sellen** say that you should "simply talk to people," that is, listen to them. Listening generated four ideas. Short experiments led to three ideas, whereas long experiments led to two ideas. Lubart & Mouchiroud are right that self-experimentation's biggest use was as an easy way to test new ideas; it was just one of several sources of ideas to test.

R4. What else mattered?

According to the target article, the rate of discovery of new cause–effect relationships was high because the work combined (a) self-experimentation and (b) new or unexploited theories. The theories narrowed the possibilities. Self-experimentation made it possible to test many of the remaining possibilities. Because a self-experiment measures many things at once, these tests often generated new ideas.

Several commentators suggest that other features shared by the examples helped raise the rate of discovery. For example, **Lubart & Mouchiroud** suggest that I was unusually motivated because I was studying "problems that [I] considered to be personally important" (para. 5). One way to determine what else mattered is to examine other discoveries. The discovery that the n-3 fatty acids found in fish are heart-protective is a convenient comparison, because Burr (2000) used it to derive the following seven rules about scientific progress.

1. "*Seek out populations that have unusual disease patterns and unusual diets and visit them*" (Burr 2000, p. 397S, italics added). Hugh Sinclair, a British physiologist, visited the Eskimos and noticed their lack of heart disease in spite of a diet very high in fat. This agrees with **Schegloff**'s belief in the value of observation and **Halberg, Cornélissen & Schack**'s (**Halberg et al.**'s) and **Grote**'s views that science and everyday life (including travel) can be profitably mixed.

2. "*Look out for natural experiments*" (Burr 2000, p. 397S), a principle which **Lubart & Mouchiroud** mention. During World War II, there was a sharp change in the Norwegian diet (less meat, more fish) and a sharp decrease in heart disease at the same time. This is more support for **Schegloff**'s belief in observation.

3. "*Use data that were originally collected for another purpose*" (Burr 2000, p. 397S). Data collected for another purpose were used to test the hypothesis that fish consumption reduced heart disease. Likewise, data collected for another purpose were useful in Examples 1–5 of the target article.

4. "*Look out for observations that do not fit in with the received wisdom*" (Burr 2000, p. 397S). When Sinclair visited the Eskimos, all fat was supposed to promote heart disease. This too supports **Schegloff**'s view of the value of observation. All of the target article examples, except perhaps 4 (morning light) and 5 (colds), contain observations that did not fit with received wisdom.

5. "*Look outside your field of research for ideas*" (Burr 2000, p. 398S). **Lubart & Mouchiroud** and **Miller** make similar points about the value of contact with other people and other areas of knowledge. The target article strongly supports this view. In Example 1 (breakfast), an animal-behavior fact (anticipatory activity) helped explain something about sleep. In Example 2 (faces), a sociological study (Sza-

https://doi.org/10.1017/S0140525X04350065 Published online by Cambridge University Press

lai 1972) suggested a circadian-rhythm experiment. Examples 7–10 show how Pavlovian-conditioning research led to new ideas about weight control. **Glenn** notes that I used operant-conditioning designs (e.g., long baselines) to study other topics.

6. *"If you can, be a subject in your own study."* Sinclair put himself on a diet of seal meat and fish for 100 days in 1976 and achieved record bleeding times as a result (Burr 2000, p. 398S, from Holman 1991). This helped to show that the Eskimo diet made a difference.

7. *"Research can be enjoyable"* (Burr 2000, p. 398S). I think Burr means that you should try to combine research and pleasure (such as travel to exotic places). This is not far from **Lubart & Mouchiroud**'s point that one reason for the success of my self-experimentation was its personal value.

So Burr (2000), discussing an unrelated discovery, agrees with several of the commentators' points. Perhaps the clearest agreement is about the value of: (a) Contact with other people and other areas of knowledge, (b) Observation, and (c) Personal involvement and/or enjoyment.

R5. What's worse than self-experimentation?

Zizzo writes well about a type of evidence that some psychologists question even more than self-experimentation namely, *mental experiments*, whose basic datum is what the data gatherer says he would do or feel in a certain situation. To many experimental psychologists, the hierarchy of data goes like this (from best to worse):

1. Property of action. Reaction time or percent correct, for example.

2. Verbal report of an internal state (**Miller**'s "private events"). Rated mood or certainty, for example.

3. Verbal report of a hypothetical action or hypothetical internal state. Statement of what one would do with lottery winnings, for example. Many decision-making studies gather this type of data (Thaler 1987).

With each type of evidence one can (a) study oneself (worse) or other people (better), (b) study one person (worse) or several people (better), and (c) make one measurement (worse) or multiple measurements (better) per subject – a total of four dimensions. Most experimental-psychology research is on the "better" side of all four: It measures actions, studies other people, studies several people, and makes multiple measurements per subject. Mental experiments are usually on the "worse" side of all four. The target-article research falls in between these extremes.

Zizzo's point, that even mental experiments – the lowest of the low – have value, agrees with common sense. We do mental experiments every day. For example: "How would I feel if I ate that?" (I am in a café). Their outcomes guide our actions. Plainly they are more accurate than chance. To treat them as worthless is to lose information that might be useful. In a subtle way, Zizzo is saying about behavioral science what Jacobs (2000) said about economies, that a society that treats certain people as outcasts loses the ability to have their work (e.g., toilet cleaning) be the seeds of economic growth (e.g., a cleaning business). Just as ostracism can slow economic growth, demands for methodological "correctness" can slow scientific progress. Over the last 20 to 30 years, few new treatments for bipolar disorder have been developed. Post and Luckenbaugh (2003) attributed this to too-high standards of evidence. "Many of us in the academic community," they wrote, "have inadvertently participated in the limitation of a generation of research on bipolar illness . . . in part by demands for methodological purity or study comprehensiveness that can rarely be achieved" (Post & Luckenbaugh 2003, p. 71).

R6. Miscellaneous methodology

Booth recommends double-blind studies. **Voracek & Fisher** recommend some sort of blinding (to eliminate bias). However, none of the target-article treatments, like many medical treatments, allows such experiments. Double-blind experiments are still used to obtain Food and Drug Administration approval of new drugs, but, as far as I can tell, they are being replaced in the research literature by experiments that compare the new treatment to one or more accepted treatments. An example is Jenkins et al. (2003), a study that measured the effect of three treatments (two accepted, one new) on cholesterol: a low-fat diet, a low-fat diet plus lovastatin, and a low-fat diet plus several foods that individually lower cholesterol.

Double-blind experiments have serious limitations. These include: recruitment difficulty (because potential subjects have a 50% chance of receiving a worthless treatment); compliance difficulty (why take a pill that does nothing?); trouble maintaining ignorance about the treatment (when the new treatment has noticeable side effects, as powerful treatments often do); the difficulty of implementing blinding and correcting mistakes in the implementation; failure to reproduce the conditions under which the new treatment would be used, if it works (if used clinically, recipients would know what they are getting); and failure to answer the question of most interest to clinicians and patients: What works best? Post and Luckenbaugh (2003) made similar criticisms. The new style of experimentation has none of these limitations. No design is perfect, but, as evidence increases that most placebos have little effect (Hrobjartsson & Gotzsche 2001; Kienle & Kiene 1997), double-blind experiments become even less desirable. Examples 6–10, taken together, are a single-subject version of the new style of experimentation: Five ways of losing weight, all believed and hoped to work, were compared. One of the diets (eating low-glycemic-index food), although new at the time, is now common.

Voracek & Fisher disagree with what they describe as my claim that "single-subject experiments do not require different inferential statistical analysis than typical, multiple-subject experimental, and observational research." Unfortunately, they do not say why. As Tukey (1969) emphasized, all experiments are $n = 1$ in many ways. One of their concerns is serial correlation ("appropriate time-series analyses are never performed"). It would be more accurate to say that they are not reported. As far as I could tell, serial correlations were too small to matter. In some cases, the data are given in enough detail so that this conclusion can be checked. When they are not, I am happy to supply the data to interested readers. As with $n = 1$, the issue is not restricted to self-experimentation. Few human experiments study all subjects at the same time. Rather, one person is tested on Monday, another on Wednesday, and so on. The time-series aspect is almost always ignored in the published analyses. Voracek & Fisher also say "the loess procedure is not appropriate for time-series data which are necessarily serially correlated." The statistical tests based on the loess

https://doi.org/10.1017/S0140525X04350065 Published online by Cambridge University Press

fits shown in Figures 14 and 15 assume uncorrelated residuals. In those figures, the abscissa is not time. The loess fits to time series (e.g., Fig. 1) do not assume zero serial correlation. They are just a way to draw a line.

According to **Voracek & Fisher,** "it is contradictory that Roberts favors visual display of data and exploratory data analysis, citing Tukey's (1977) seminal work, and at the same time reports numerous inferential statistical tests." Tukey (1980) emphasized the need for both sorts of analyses.

Voracek & Fisher wonder about confoundings: "Roberts seems to have performed multiple experiments simultaneously, and therefore, the conclusions are suspect." Unfortunately, they do not give examples. Dates given with the figures can be used to establish when various changes were made and thereby reassure oneself that one experiment did not interfere with another.

Miller says that I "produced no methodological breakthroughs in measuring" mood and how rested I felt. Quite true. Economy of means (one subject, simple designs, simple measurements, low cost, low tech, no lab) is no breakthrough, but it is a virtue.

R7. Evolutionary psychology

Miller asks: Is the target article evolutionary psychology? **Glenn** notes that many of the core ideas are evolutionary, but that it differs from previous evolutionary psychology because "the experimental method [is brought] to bear on hypotheses heretofore examined only in terms of correlational research."

What is evolutionary psychology? Buss et al. (1998) provided a definition by listing "thirty recent examples of empirical discoveries about humans generated by thinking about adaptation and selection" (p. 544). The target article's Examples 2 (faces) and 3 (standing) were generated by thinking about evolution. The key concept, however, was not adaptation or selection, but mismatch, the idea that our way of life can change much faster than our genes. An evolutionary idea was some of the initial support for the weight-control theory proposed in Example 7, and that theory led to Examples 7–10.

The target article differs from Buss et al.'s (1998) "thirty recent examples" in several ways:

1. As **Glenn** says, it is experimental. Almost all of the Buss et al. examples are correlational.

2. The theory of Examples 1–5 assumes maladaptation, that what happens now (e.g., early awakening) is *not* what happened in the Stone Age. The theories behind all of the Buss et al. examples seem to assume the opposite: What we currently observe is more or less what happened in the Stone Age.

3. Examples 2 and 3 and the weight-control theory have obvious practical applications, unlike any of the Buss et al. examples.

So the target article offers new support for evolutionary theorizing.

Questioning my evolutionary explanation of Example 2 (faces), **Totterdell** asks, "Why is a mood rhythm required to synchronize mood when there are many other more immediate unconscious and conscious processes known to bring about mood synchrony in groups (Kelly & Barsade 2001)?" My argument that the mood rhythm developed because it helped synchronize mood does not assume this rhythm was needed to synchronize mood; it just assumes such synchrony was beneficial.

Different mechanisms with the same effect are common, even ubiquitous, in biology. The hedonic shift noticed by **Cabanac** (water that usually felt unpleasantly cold felt good when he was hot) helps cool us when we are hot; so does the fact that we sweat more when hot. Many enzymes repair DNA (Sancar & Sancar 1988). Multiple mechanisms help us adjust to dim light (Woodhouse & Campbell 1975). We locate sounds using more than one mechanism (Middlebrooks & Green 1991). Because multiple mechanisms with the same effect are common, they can be used to assess evolutionary explanations. In the examples just given, the outcomes (constant body temperature, DNA repair, and so on) are obviously beneficial. If there are multiple mechanisms that produce Outcome X, it is more plausible that X was good (beneficial under Stone-Age conditions) than that X was bad. So the existence of multiple mechanisms that produce X can be used to argue that X was good. For example, suppose women prefer rich husbands to poor ones. An evolutionary explanation is that this preference is genetic and evolved because it gave the children of women with such genes more resources – supposedly a good thing. The assumption that more resources for one's children is good becomes more plausible if there are other mechanisms with the same effect (more resources for one's children). It becomes less plausible if other mechanisms have the opposite effect (less resources for one's children). According to this reasoning, the existence of other mechanisms that synchronize mood (Kelly & Barsade 2001; Totterdell et al. 1998) *supports* my assumption that the synchrony was beneficial. As far as I know, this is a new way to judge evolutionary explanations (see Buss et al. 1998 and Andrews et al. 2002 for the usual ways) – for which we can thank **Totterdell.**

R8. Mood

Totterdell is perhaps the ideal commentator on the mood results. He has studied mood with great sophistication (e.g., Totterdell et al. 1994), and his observations of his infant daughter (Totterdell 2001) resemble self-experimentation in several ways. "Both morning and evening faces," writes Totterdell, "caused a trough in mood about 18 hours after the stimulus" making it "plausible that the mood oscillation was dependent only on the social zeitgeber rather than being gated by a light-sensitive clock." One reason for assuming gating by a light-sensitive clock is that changing the timing of morning faces by just one hour in either direction diminished their effect (target article Fig. 8, upper panel; Roberts & Neuringer 1998). Faces in the late morning and afternoon had no effect, something I noticed many times but did not show in an experiment. More evidence for the involvement of a light-sensitive clock is that travel across time zones temporarily eliminated the effect (target article Fig. 9).

Totterdell wonders whether "seeing faces during Stone-Age sexual activity [was] also confined to the early morning." I suspect, but did not show, that seeing a face very close has little effect. Viewing distance clearly matters (Fig. 7; Roberts & Neuringer 1998).

Totterdell points out that "models based on the joint action of a circadian clock and a sleep need process have also

https://doi.org/10.1017/S0140525X04350065 Published online by Cambridge University Press

been very successful in predicting levels of alert mood." I measured not alertness but happiness, serenity, and eagerness. Alertness is about noticing stimuli; eagerness is about initiating action. As far as I can tell, I was quite alert almost the entire time I was awake, while my eagerness varied greatly (target article Fig. 5, lower panel).

I think I understand **Moore & Sellen**'s proposal: The very first morning I felt unusually happy (the morning of the accidental discovery), my mood was high due to chance. I was happy on subsequent days due to the (false) belief that I had discovered something. It is unclear to me how this idea explains other findings, such as the biphasic shape of the effect (Fig. 5, lower panel).

Moore & Sellen would presumably predict that the effect would not be repeatable with anyone else because it would not be a discovery. Other commentators have other doubts. **Booth** believes repetitions of the basic effect are "contaminated by . . . [my] knowledge of previous observations." **Totterdell** wonders if the effect depends on my "abstinence from evening interaction" and "sleep difficulties." **Voracek & Fisher** say "there is a large history of errors through self-inspection and self-experimentation" and wonder about generality and individual differences. For information about replications of the basic results, see http://psychology.berkeley.edu/pdf/roberts-replications.pdf.

R9. Weight

Booth believes that the set-point idea is wrong, or "redundant." He prefers to think of opposing negative feedback functions, which is what I think of when I try to imagine how a set point is produced. For more about the set-point concept, see Cabanac and Gosselin (1996) and their 26 references, both pro and con. The broader problem for Booth is that, in spite of what he considers my mistakes, I found a very effective and counterintuitive way of losing weight (Example 10). About this, all he says is "a lot of fructose without glucose is poorly absorbed and the resulting upset could reduce hunger." I do not recall any upset stomachs, and the daily amount of fructose was neither large nor consumed all at once. Booth knows so much about food, eating, and appetite, I can only assume his heart wasn't in thinking of an alternative explanation of Example 10.

Miller and **Voracek & Fisher** question the generality of the results. Miller wonders if the striking effect of fructose water (Example 10) depended on my earlier weight-loss experiences (Examples 1, 6–9) or other unusual aspects of my life. Moreover, according to Voracek & Miller, "there is a large history of errors through self-inspection and self-experimentation." After the target article was written, I learned of several rat studies where sugar (sucrose) water caused great weight loss (Hamilton 1971; Hamilton & Timmons 1976; Hamilton et al. 1980; Sclafani 1973). For information about the experience of those who have tried the diet, see http://psychology.berkeley.edu/pdf/roberts-replications.pdf.

One person's experience is worth describing here because it makes a larger point. A *BBS* reviewer tried the sugar-water diet and noticed that, at a dose that was "considerably lower" than my lowest dose, the fructose caused "pain in joints that were ordinarily pain-free." The reviewer had recorded joint pain for 20 years and had found that "distinctive patterns of pain accompany certain additives in food." The pain went away when each additive was avoided and returned within hours when it was consumed again. "Using a standard design of fructose, no-fructose, etc., in a matter of two weeks it was clearly demonstrable that I have a strong sensitivity to fructose (at least in the powdered form I used)," the reviewer wrote, which was "quite surprising." The reviewer concluded that self-experimentation by "an informed citizenry," plus aggregation of the results, similar to what **Halberg et al.** do with blood pressure, "may be the only way for us to ever recognize the full range of differences among individuals on health and psychological dimensions."

R10. How difficult is self-experimentation?

At least two of the commentators believe that self-experimentation similar to what the target article describes is very difficult. "The number of readers who are prepared to commit themselves to such a path in the future," writes **Schegloff,** "is surely limited, to say the least." **Miller** says the same thing obliquely when he kindly states that the work "may even constitute heroic achievement" and that some parts "inspire awe."

Perhaps it *was* heroic to drink 5 liters of water per day (Example 6). Aside from Example 6, however, none of it seemed like much trouble. Almost all the treatments took advantage of what was going to happen anyway. In Example 1 (breakfast), I was going to eat breakfast anyway; I just varied what I ate. I was going to sleep anyway; the sleep measurements took only a few seconds per night. For Example 2 (faces), I watched the faces while I walked on a treadmill, which was exercise; I was going to exercise anyway. The average reader probably cringes at the thought of standing 8 hours per day (Examples 3 and 5), but I was already going to do several things, such as work and watch TV, that could be done standing rather than sitting if I changed the environment (e.g., raised the TV). After my legs became stronger, standing 8 hrs/day was not painful. Fluorescent light exposure (Examples 4 and 5) happened while I walked on a treadmill watching TV, that is, during exercise. Eating lots of sushi (Example 9) was cumbersome and expensive but pleasant as well. The target-article research can be seen as a blend of previous self-experimentation, which was more lab-like (e.g., Ebbinghaus 1885/1913), and epidemiology, which takes advantage of preexisting differences.

On the other hand, most of the target-article treatments were more difficult than what is asked of most experimental subjects. Standing 8 hrs/day (Examples 3 and 5), drinking 5 liters of water per day (Example 6), and eating diets of pasta (Example 8) or sushi (Example 9) for weeks are considerably more difficult tasks than what subjects do in a typical sleep or weight experiment. **Lubart & Mouchiroud** correctly say that the problems I studied were "personally important," so I was "highly motivated to solve [them]." But it was more than that. A big reason why I persisted, and did relatively difficult things, was that prior self-experimentation had been successful. Success of the weight-loss treatment of Example 1 (eating less-processed food) motivated Examples 6–10. That skipping breakfast in Example 1 reduced early awakening motivated Examples 2 (morning faces), 3 (standing), and 4 (morning light). The short-term successes of Examples 3 and 4 motivated the

https://doi.org/10.1017/S0140525X04350065 Published online by Cambridge University Press

long-term changes and record-keeping of Example 5. Compared to the benefits, in other words, the research was not hard at all. I slept much better (Examples 1, 3, and 4). My health greatly improved (Example 5). My mood improved (Example 2). With little suffering, I reached a weight I considered ideal (Examples 1, 6–10). I went from eating three meals a day to eating one a day, saving about 2 hrs/day. In some ways, the large benefit/cost ratio is the most surprising feature of this work.

Bernstein (1997) experienced a similar benefit/cost ratio – a lifetime of much better health and freedom from diabetic complications, at the cost of about a year of not-difficult self-experimentation. In both cases, Bernstein's and mine, earlier work by others was crucial to our success. Bernstein could not have done what he did without the discovery of insulin, not to mention the blood-glucose meter he used and basic research in nutrition and physiology; and in a similar way, my work depended on the creative work of many people. Example 2 (faces) could not have happened without Wever (1979) and Szalai (1972). Example 10 (sugar water) could not have happened without Ramirez (1990a), not to mention Pavlov, Sclafani, and Cabanac. In Bernstein's case and mine, it is hard to avoid concluding that our self-experimentation (small, quick, flexible) had a large benefit/cost ratio because it found the last piece of a puzzle, a piece that (big, slow, inflexible) conventional research could not easily find.

R11. The market for self-experimentation

According to Christensen (1997), advances in technology have often caused the leading companies within an industry to lose their lead. It happened several times in the disk drive industry as drives became smaller. The most profitable companies when most drives were 14 inches in diameter lost their lead when 8-inch drives became popular. Several folded. The same thing happened when 5.25-inch drives were introduced and again when 3.5-inch drives came along. In the mechanical-shovel industry, starting around World War II, there was a shift from cable-driven shovels to hydraulic ones. Few of the firms that made cable-driven shovels survived the transition. In these and other examples, Christensen identified a common thread: The industry-leading firms were "held captive by their customers" (p. 18). They failed to appreciate the new technology because the new technology did not – in the beginning – do what their customers asked for. At first, the capacity of 8-inch drives was inadequate for the mainframes owned by the buyers of 14-inch drives. When they began, makers of 8-inch drives (and 5.25-inch drives and 3.5-inch drives) had to find *new* customers for disk drives, such as makers of minicomputers, desktop computers, and laptop computers. As the smaller drives improved (as the capacity of 8-inch drives increased, for example), they came to dominate the market previously occupied by the larger drives. When hydraulically driven mechanical shovels were first introduced, they were too small for most construction jobs. They were sold as tractor attachments; they did very small jobs previously done by hand, such as digging ditches from water lines in the street to the foundations of new houses. To the makers of big shovels, digging tiny ditches seemed like a trivial market. They saw no threat. But the maximum size of hydraulic shovels slowly increased. Eventually they could dig much larger holes and this spelled doom for the makers of cable-driven shovels. In these and other examples, the new product was seriously deficient when it first appeared. The first 8-inch disk drives did not store enough data. The first hydraulic shovels held too little dirt. But with the help of new customers, who wanted different features than the customers for the older product, the new product survived and improved and eventually replaced the older product. Christensen concluded it is unwise to dismiss a new version of a familiar product just because you (or your customers) do not want it. Others may want it. With experience, it may improve.

Self-experimentation, it is safe to say, is considered of little or no importance by the vast majority of behavioral and biomedical scientists (e.g., Jasienski 1996). Judging by publications, it is very rare. It has serious limitations compared to more traditional experimentation. The commentators sum up these limitations: the possible influence of expectations and desires (**Booth, Voracek & Fisher**), lack of information about generality across persons (**Miller,** Voracek & Fisher), and a limited range of application (Voracek & Fisher). But if self-experimentation can find even a small market, then experience can lead to improvement.

For many years, the scientific uses of self-experimentation were mainly medical. Doctors tried dangerous or difficult treatments on themselves (Altman 1987/1998). **Booth**'s half-pound-chocolate lunch is in that tradition. To an experimental psychologist, one person doing something once to see what happens is hardly science (just as digging tiny ditches is hardly construction work). Science or not, it persisted. Surely it will never be common, any prominent doctor or scientist would have said. But in 1969, as the target article mentions, Richard Bernstein, concerned about his own diabetes, obtained a new blood-glucose meter that required only one drop of blood. He used it to intensively study the effect of insulin and diet on his own blood glucose levels. This is closer to an experimental psychologist's idea of "good science," that is, there was a lot of repetition and careful study of cause and effect. Of course, it could be dismissed as therapy, not science, with results helpful only to Bernstein. But the second half of this dismissal would be wrong. His work led to the current use of blood-glucose meters by millions of diabetics, who practice a simple form of self-experimentation when they use meter readings to adjust their diet and insulin use. I know four people who use these meters. One said it was the greatest advance in diabetes treatment since the discovery of insulin. Few researchers have helped more people than Bernstein. **Voracek & Fisher**'s diabetic friends would not agree that the precursors I cite "include no major or recent findings" and that the results of self-experimentation have "limited application."

Christensen (1997) distinguished two ways that sales can expand: *downmarket* and *upmarket*. Going downmarket means finding entirely new customers for the product, who are inevitably less profitable and prestigious than the old customers, at least initially. For example, selling tractor attachments was less profitable and prestigious than selling giant shovels; selling 8-inch disk drives to minicomputer users was initially less profitable and prestigious than selling 14-inch disk drives to computer centers. Going upmarket means capturing customers from the older version of the product (e.g., when a cable-driven shovel is replaced by a hydraulic one). Going downmarket always happens first, according to Christensen; experience gained with the new customers helps penetrate the older market. Bernstein took

https://doi.org/10.1017/S0140525X04350065 Published online by Cambridge University Press

self-experimentation downmarket. He found entirely new users of experimentation (persons with diabetes). This had no effect on the usual users (scientists), but it greatly reduced doctor's visits, just as hydraulic-shovel tractor attachments had no effect on big contractors, but greatly reduced muscle-powered ditch-digging. One indication of the lack of profit and prestige in what Bernstein did is that hardly anyone has heard of him. The target article takes self-experimentation upmarket, in the sense that it is published in a mainstream journal (displacing an article based on traditional methods) and deals with mainstream scientific questions.

Totterdell and **Glicksohn** propose ways that self-experimentation might move further upmarket (further replacing more conventional science). Totterdell suggests that journal space be devoted to "new ideas," which would probably mean more space for self-experimentation. Glicksohn shows how self-experimentation can be used to answer important methodological questions; in his particular case, the optimal epoch size for analysis of EEG data. His example is from an experiment with two subjects rather than one, but that hardly matters. His broad point is that an experiment with one subject can help you do a later experiment with several subjects. The first experiment (with $n = 1$) can choose a procedural parameter or method of analysis; it can also generate a prediction. In these cases, to present only the second experiment (with $n > 1$) is to substantially understate the evidence. Any result is more impressive if predicted, but the basis of the prediction needs to be spelled out.

Halberg et al. and **Grote** propose ways that self-experimentation could move downmarket (into lower-prestige areas where many scientists would not deign to go). The first disk drives were found mostly at large companies and colleges – places with computer centers. Now they are in millions of places, an expansion made possible by smaller disk drives. Halberg et al. and Grote suggest the expansion of experimentation (or at least science-like observation) into the lives of millions of non-scientists. The expansion is possible because of the small "size" of self-experimentation. Halberg et al. emphasize prognosis. They show that long-term ambulatory measurement of blood pressure, done outside of medical settings, has predictive value. It is a small step from using such measurements for prognosis to using them to guide treatment. Grote emphasizes treatment. She extends what Bernstein did to more behavioral problems, especially those involving self-discipline, such as smoking cessation. To prevent relapse, Grote says, it may help to give individuals more control over their treatment. Just as a diabetic adjusts diet and insulin dose based on blood-glucose readings, so someone who is trying to stop smoking might adjust her activities based on self-measurement of her desire to smoke. No one believes that one size fits all in the treatment of diabetes or, probably, anything else; Grote argues, as Bernstein successfully argued, that a lot of the fitting is best done by the person being fit.

History is on the side of **Halberg et al.** and **Grote.** Several scientific activities have spread into the general public: (1) *Literature searches.* Non-scientists make heavy use of Medline, the National Library of Medicine's online retrieval system (Cline & Haynes 2001). (2) *Result sharing* (emphasized by **Totterdell**). Support groups and Internet discussion groups for diseases and other problems include considerable sharing of experience – how well a certain treatment worked, for instance. (3) *Sophisticated and systematic measurement.* Blood glucose meters, blood pressure meters, and cholesterol meters are available to the general public. A *BBS* reviewer of the target article mentioned BirdSource, described on its website (*http://www.birdsource.org*) as "a revolutionary partnership between citizens and scientists." BirdSource allows bird-watchers in different parts of North America to combine their observations to learn about bird migration, population, and conservation. All three activities (literature searching, result sharing, and sophisticated and systematic measurement) are growing, as far as I can tell; for example, Medline searches by the general public have greatly increased in recent years (Cline & Haynes 2001), the price of cholesterol meters is decreasing, and BirdSource began around 1997.

Another step in the direction of more self-experimentation (and possibly another example of the "first downmarket, then upmarket" pattern) is the growth of *n-of-1 trials,* which are randomized controlled clinical trials with just one patient. The original idea was that clinicians would use such trials to find the best treatment and dosage for a patient (Guyatt et al. 1990; Johannessen et al. 1990), an improvement over simple trial and error (which is how I originally viewed self-experimentation – as an improvement over simple trial and error). *N*-of-1 trials were, among other things, a way to follow-up large randomized controlled trials, which "all too often . . . are carried out on populations so ill-characterized that clinicians cannot be sure whether it is appropriate to extrapolate results to the individual patient confronting them" (Price & Evans 2002, p. 227). More recently, though, *n*-of-1 trials have been proposed to *replace* large randomized controlled trials (Post & Luckenbaugh 2003).

Human nature is also on the side of **Halberg et al.** and **Grote.** Before science was a profession, it was a hobby, which means some people enjoy it for its own sake (and which BirdSource takes advantage of). If a hobby has tangible benefits, such as lower blood pressure or reduced risk of relapse, so much stronger the motivation to do it.

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Letters "a" and "r" appearing before authors' initials refer to target article and response respectively.

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https://doi.org/10.1017/S0140525X04350065 Published online by Cambridge University Press