

## Response to Commentaries

### Obstacles to Studying Real People Eating Real Meals in Real Situations

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When I wrote my paper, I hoped to express my concerns about the field of human eating research, to stimulate discussion about methods, and to be a catalyst for some future work. The commentaries on my paper form an initial burst of discussion of human eating research which will continue below and hopefully well beyond today. Readers will be struck, as I was, by the diversity in responses. In my opinion, this diversity is positive because it denotes the lack of a dominant view in the field and, hence, greater potential for further development.

I argued for a balance of laboratory and non-laboratory research, stating "clearly some studies are best done, or can only be done, in laboratories . . . It is not the presence of laboratory research which I am trying to change, but the ratio of laboratory to field research". Some commentaries believed that I see non-laboratory research as the panacea for all problems. Of course not; I want to achieve a balance which does not exist now. Most of the commentaries agree with the basic goal of more research in a broad range of situations. Further, many studies which are best done in the laboratory should be followed up by field studies, a pattern which does not exist now (Mela *et al.*, 1992).

However, many commentaries see field work as limited in potential. Stellar (1992) and Booth (1992) both see past field studies of limited help in understanding eating behavior. If that is the case, it might be because there has been so little. After there has been more non-laboratory work we will be able to reassess its contribution. But a number of commentaries regard non-laboratory studies as observational (Booth, 1992; Mela *et al.*, 1992; Tuorila & Lähteenmäki, 1992; Rolls & Shide, 1992; Stellar, 1992) rather than manipulative. Some other commentaries see an adequate amount of field research, but not in the fields tapped by this journal. Without getting into the argument of whether or not dieticians, nutritionists, and consumer researchers study real people in real eating situations, those interested in the issues covered by this journal do not. Mela *et al.* (1992) called for more in *Appetite*. In the same vein, some demand research which is hypothesis-driven (Kissileff, 1992; Rolls & Shide, 1992) implying that non-laboratory research cannot be. Field research is not limited to observation and can be hypothesis-driven. I believe that some confuse my call for more non-laboratory or field research as a call for more questionnaire studies without direct observation and manipulation of eating. I have recently

conducted studies varying effort to obtain food in a British student cafeteria which is manipulative and hypothesis-driven (Meiselman *et al.*, 1991). Also, Engell *et al.* (1990) studied the effect of social influence on food intake in a military cafeteria. Similarly, foods, social context (vary table size, vary number of people, etc), food cost, and other variables can be manipulated. Kissileff asks whether non-laboratory studies can use unobtrusive measures of food intake. Krantzler *et al.* (1982) demonstrated unobtrusive and other intake measures in a student cafeteria and we have used similar methods in a number of studies.

Kissileff suggests that a distinction be made between studies of amount eaten and studies of what is selected to eat and the time and place of eating. He proposes that the laboratory is the appropriate place to study "what controls how much people eat and when they stop eating", although he also suggests studying eating controls in different environments. I do not follow Kissileff's argument that amount eaten must be studied in the lab. If we are to study where you eat, when you eat, and what you eat in natural eating situations, then why not study how much you eat there also? How much you eat could be a function of these other situational factors.

An interesting theme in the commentaries centers on what is real? I recommended that research use "real-life eating situation(s)" and "real meals or diets constituted as the subjects would choose". By my definition, if it exists in the real world without the imposition of researchers it is real. No one eats in the laboratory other than by the design of researchers. By this definition, lab meals are not real. The commentaries question what is real eating (Kissileff, 1992; Rolls & Shide, 1992; Pliner, 1992) and Mela *et al.* (1992) even suggest that institutions are my laboratory and are also not real. I recommend that we accept as real any place where people normally eat. Kissileff provides excellent information on how to approach this issue in his commentary.

There is a general consensus that we need to know the differences in eating behavior in different situations, including lab-field comparisons (Stellar, 1992; Pliner, 1992; Rolls & Shide, 1992; Tuorila & Lähteenmäki, 1992; Kissileff, 1992). That is exactly what I would like to know. When human eating research is so dominated by laboratory research this is not possible. Pliner also suggests that laboratory eating need not be so constrained, and this opens up other possibilities for study.

But there is also a range of concern about methods for field research. Virtually all commentaries asked how to do such research or criticized some field approaches. Rolls & Shide see field methods as "not very precise" and "very expensive". Kissileff decried the absence of unobtrusive measures, Tuorila & Lähteenmäki decried the dependence on self reports, and Booth decried lumping customers together. As with laboratory research, field research can be designed to be expensive or inexpensive, more or less precise, self-report or observational, etc. No one paradigm of field research is appropriate, just as no one paradigm of laboratory research is appropriate. There was equivalent concern about where to test. Unfortunately I had mentioned hospitals and prisons as examples, and they, along with the military, became favorite targets. It *is* easier to conduct laboratory research, especially repetitive studies using the same paradigm. But doing field research as part of a total research program is worthwhile in my opinion.

The latter sections of my paper challenged four current emphases of human eating research: 1) shorter term studies, 2) senses and physiology, 3) animal studies, 4) abnormal eating. Few commentaries directly dealt with these. Kissileff, Mela

*et al.*, and Stellar argued to keep the study of physiological controls in the lab. Tuorila & Lähteenmäki argued that sensory factors are more important in the development of food preferences than in everyday food choices. And Kissileff agreed that understanding normal eating is critical to understanding abnormal eating. I hope that future reviews and commentaries will deal with these issues further.

Several commentators mentioned the relative lack of theoretical issues in my paper, given its title on "Methodology and theory . . .". The ways in which we study human eating limit our identification of variables and our understanding of how these variables impact eating. In the laboratory many situational variables are absent or function differently. In this way, the preponderance of laboratory research affects our theories of what controls human eating. I believe this is why we have not been terribly successful at attempts to control human eating, because our conceptualization of it is inadequate. Our conceptualization is based on brief snapshots of eating, often with non-realistic foods in non-realistic situations. When we change our ways of studying human eating, we will develop new and more complex models of eating and new ways to change eating behavior.

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