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Response Times

Their Role in Inferring Elementary Mental Organization

R. Duncan Luce

RESPONSE TIMES

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R. DUNCAN LUCE

Victor S. Thomas Professor of Psychology
Harvard University

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Dedicated to Those of My Ph.D. Students
Who Have Contributed to Our Understanding
of Response Times:

Stephen L. Burbeck

Robert T. Ollman

Joan Gay Snodgrass

Saul Sternberg

Brian A. Wandell

Preface

For almost as long as I have been doing research, response times have struck me as a fascinating, albeit tricky, source of information about how the mind is organized. Whenever I teach mathematical psychology or psychophysics, I include a dose of response times along with the repeated admonition that we surely do not understand a choice process very thoroughly until we can account for the time required for it to be carried out. When I came to Harvard in 1976 I offered, for the first time, a seminar-course on the subject (in style more a course, in size more a seminar), first with David Green and later alone. It was only then that I felt a need for a more systematic mastery of the field, and in academic 80–81 when I had a sabbatical leave, the Guggenheim Foundation agreed to support my self-education and the beginnings of this book.

Gradually, with much rewriting and reorganization as I came better to know those parts of the area I had previously slighted, the book has evolved. It has been four years in the making, slowed considerably by two facts: that I was department chairman during three of those years and that I have maintained an active research program, with Louis Narens, in axiomatic measurement theory.

My attempt is a critical, but even-handed, treatment of the major themes of how response times play a role in out thinking about the mind. I am quite aware of my tendency, apparently not wholly idiosyncratic, to be more protective of and sensitive to the nuances of those topics in whose birth and nurturing I have participated. I can only hope that the reader will discount my biases accordingly.

One bias, not easily overlooked since it pervades the book, is my preference for hypotheses formulated as explicit mathematical models and for developing their logical implications for experiments. That fact necessarily raises questions of mathematical prerequisites and about the general mathematical level of the book. Let me try to answer that explicitly. I assume familiarity with elementary calculus and with elementary probability theory, both of which are used freely as needed. Chapter 1 attempts to outline all of the probability ideas that are used, and I believe that with some diligence on the part of the reader, it should suffice for all but small parts of Chapters 3 and 4, much of Chapters 8 and 9, the first part of Chapter 11, and all of Appendix A. Within the rest of the book, there are extensive empirical sections that demand nothing beyond logarithms, power functions, and the like. This least mathematical material includes all of Chapters 2 and 6, a good deal of Chapter 10, and parts of Chapters 11 and 12. So, put another way, the book really lives at three different mathematical levels, and the reader can expect to learn something additional at each level.

The work has benefited greatly from the contributions of others. Most important is what my students, both in seminar and doing Ph.D. dissertations, have taught me. As the book has developed, some students and professional colleagues have been exposed to early drafts and have provided feedback. Especially important were two sources of comments. One was the incisive and accurate marginal notes of Donald Laming who, more often than I like to admit, rooted out errors in my text and took reasoned exception to my views. I have gratefully corrected the errors, but my views have not always proved equally responsive. The other was a series of seminars conducted at the Bell Telephone Laboratories in Murray Hill, N.J., under the auspices of Saul Sternberg. His enormous erudition and analytical ability have led to marked improvements in both coverage and interpretation. Beyond his comments, those of several who participated in the seminar were of immense value to me and deserve explicit mention: George W. Furnas, James C. Johnson, Ronald Knoll, Thomas K. Landauer, Robert Ollman, Steven E. Poltrock, and C. E. Wright. Others who have provided detailed and useful written comments on parts of the earlier manuscript are F. Gregory Ashby, John Baird, Thomas Hanna, Stephen Link, David H. Krantz, A. A. J. Marley, James Townsend, John Tukey, and Elke Weber. Nobody has actually seen the book whole, for it has been in flux until the final weeks of revisions. And certainly no one but me is culpable for its remaining weaknesses, but I can assure the reader that its failures would have been more severe without the help of all these people, whom I thank.

At a practical level, the Guggenheim Foundation along with Harvard University financed the beginning, sabbatical year. After that there was no direct support of the project, but the National Science Foundation and Harvard University have, for other reasons, placed some secretarial help at my disposal that has been useful. Most important, both institutions have contributed to my having microcomputers on which many chapters have been drafted and all of the revisions have been made. During the final two months of revisions, I have been on leave from Harvard at the AT & T Bell Laboratories in Murray Hill, N.J., and some of my time has been devoted to this book. A. F. Smith and R. M. Nosofsky programmed and ran a number of computations for me, Ellen Rak photographed figures from articles, Annemarie Wrenn drafted original figures, and Sandra Susse and Zoe Forbes did the bulk of the typing before I had word processing. They all receive my thanks.

Those on whom the writing has impinged personally and whose patience has no doubt been strained as I have hunched over my IBM PC are my daughter, Aurora, and my close friend Carolyn Scheer. Their good cheer has been appreciated.

Now, I only hope that you find our efforts worthwhile.

Cambridge, Mass. R.D.L.
November 1984

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Representing Response Times as Random Variables

1.1 THE STUDY OF RESPONSE TIMES

1.1.1 Why?

Response time is psychology's ubiquitous dependent variable. No matter what task a subject is asked to do in an experiment, its completion always takes time. That obvious fact is not, however, necessarily interesting, and until relatively recently psychologists have not typically recorded response times. Should response times be ignored or do they tell us things we should like to know?

The notion that response times might reveal information about mental activity is old. For example, in 1868 F. C. Donders suggested that one could infer the time taken up by a particular hypothetical mental stage by subjecting the subject to two procedures that differed only in whether that stage is used. Joseph Jastrow, in an 1890 volume entitled *The Time Relations of Mental Phenomena*, stated more generally one major argument for examining response times. If the processing of information by the mind is highly structured, as most psychologists believe, then different paths through that structure will entail different time courses, and those differences will be reflected in the response times. Thus, perhaps, one can infer back from the pattern of response times obtained under different experimental conditions to the structures involved. To the extent this is possible, response times are valuable. My aim is to survey what we know about the feasibility of that strategy.

Let me admit at the outset that there are reasons to be skeptical of the enterprise. Consider the task of inferring the architecture of a computer from measurements of its performance times using different programs and different inputs. This certainly would be difficult, especially if one lacked the technology of modern electronics to help carry out the measurements. At best, one would expect to learn something about the gross organization of the computer, but it seems unlikely that the fine details would succumb to such an attack.

So, as psychologists, we can hope at best to learn something about overall organization and very little if anything about the details. That presumably will develop only as we look, in some fashion or another, inside the "black box." This means using physiological observations, which traditionally have taken either of three approaches—two of which are largely restricted to animals and the third is better suited to human subjects. In ablation studies entire regions of the brain are destroyed in an attempt to gain some understanding of the role of that region. In single unit studies, the electrical activity of the individual neurons is observed. In the third tradition non-intrusive electrical scalp measurements are taken. Recently these methods have become far more powerful through the use of computer techniques to make inferences about average electrical activity in different regions. Although physiological results to some degree inform model building, they usually are secondary to questions of fitting the models to behavioral data; such physiological data will not loom large in this book.

From the first attempts to use response times until the growth of modern cognitive psychology beginning, say, in the mid 1950s, response times were largely the focus of specialists and were not usually recorded by others. The specialists, who tended to come from the tradition of psychophysics, were mostly interested in the limits of human performance; they spoke of studying "reaction" times.* The term was appropriate—they asked how long it took a subject to react to a signal onset (or offset) under various conditions of observation. A prime reason reaction times were a matter for specialists was the sheer technical difficulty of carrying out the measurements with the equipment of the time. It is difficult, although not impossible, to measure times of a few hundred milliseconds to an accuracy of a few milliseconds with pre-World War II technology. The past 35 years have seen electronic developments that have made such observations, if not easy, at least highly feasible.

The other major change, which also took place after World War II, was the striking shift from a strongly behaviorist-operational orientation to a cognitive one. The idea of postulating an internal mental structure that, on the one hand, has properties about which we can make inferences from external observations and, on the other hand, provides a compact theoretical summary of what we know, has passed from being an anathema to being the mainstream orientation at many major centers of psychology. Ease of measurement and philosophical adjustment has led to very extensive use of response times as a crucial dependent variable.

1.1.2 How?

In practice, the use of response times to infer something about how we process information is a good example of the interplay of experimental

* Although the current literature does not attempt to make a distinction, I shall distinguish between reaction and response times. In my usage, response time is the generic term and reaction time refers only to experiments in which response time is made a major focus of attention for the subject. The experimenter may request the subject to respond as fast as possible, or to maintain the response times in a certain temporal interval, and so on. I may be the only person who attempts the distinction. For example, the recent volume by Welford (1980) that covers many of the same topics as I do is titled *Reaction Times*, and that usage is characteristic of much of the cognitive literature. Nevertheless, I feel that the distinction should be made until we are more sure than we are now that it does not matter.