Preface

The divorce rate in the United States has reached epidemic proportions. For first marriages, demographers Martin and Bumpass (1989) estimated that within a forty year span 67% would end in divorce. However, this is not a recent phenomenon; rather, it is a trend that has been in effect for at least a century (Cherlin, 1981). The rest of the world is not far behind the United States.

Furthermore, at this juncture the phenomenon is not well understood. Although sociologists have shown that the phenomenon of increasing divorce rates is related to the world-wide overdue economic and psychological emancipation and empowerment of women, it is not understood why some marriages end in divorce, but others do not. Of all the studies on divorce, only a handful have been prospective, longitudinal studies. These few studies have generally yielded little information about why some marriages end in divorce, but others do not.

Marital therapy is also at an impasse. The effects of marital therapy are owing primarily to the massive deterioration of distressed marriages in no-treatment control groups and not to the clinically significant gains of couples in treatment groups. Only approximately 35% of these treated couples make clinically meaningful gains in treatment, and within a year or two most of these couples relapse.

In our view, the field of marriage research is in desperate need of theory. Without theoretical understanding of key processes related to marital dissolution or stability, it will be difficult to design or evaluate adequately any new interventions. Personally we have the most respect for theories that are mathematical. We believe that scientific progress will be facilitated by mathematical models.

Using a multimethod approach to the study of marriages, our laboratory was fortunate in having been able to predict the longitudinal trajectories of marriages with a great deal of accuracy, more than 90% accuracy in three longitudinal studies. About five years ago, when we first discovered our ability to predict divorce and marital stability, Gottman began exploring alternative approaches to building mathematical models of this prediction, taught a seminar on the topic, and edited a book on the analysis of change. He was in a science book club that at one point featured James Murray's book, *Mathematical Biology.* After becoming enlightened that such modeling was proceeding at a rapid pace in many biological sciences, he invited James Murray to lecture in the seminar. A letter was sent to Oxford University, but it turned out that Murray had recently moved to the United States, and had actually begun to work at the University of Washington, where Gottman also was teaching. The two of them met at the faculty club with Gottman's graduate student Regina Rushe, and Gottman proposed a project of modeling the divorce prediction data. Murray was intrigued, and he agreed to discuss the idea with his students.

A work group was formed that included Julian Cook, Rebecca Tyson, Jane White, and Regina Rushe, James Murray, and John Gottman. This group met weekly for four years, watching tapes from Gottman's laboratory, talking about what was known about marital process, building a model, and eventually analyzing data. From this interaction, which Murray guided, the mathematical model of marriage emerged.

The model was remarkable. Immediately it gave us a new and parsimonious language for describing a marital interaction. It also gave us a set of parameters that we discovered were predictive of marital stability and marital quality. We have also applied the model to the study of preschool children interacting in six-person groups (Gottman et al., 1995). The model also worked well in that context, so we began thinking that perhaps the model was quite general in its representation of social processes.

As we modified our procedures and coding of the videotapes of couples in order to obtain more data points per interaction and to obtain them more rapidly, we modified the model. We then discovered new parameters that distinguish happily and unhappily married couples, such as the level of negativity required before a spouse reacts. We called this parameter the negative threshold effect. We began thinking about the relationship of this parameter to the fact that people delay getting marital therapy for an average of six years (Notarius and Buongiorno, 1992), and to the pervasive relapse effect in marital therapy. We began conceiving additional modifications to the model - for example, including a repair term that could explain why negativity does not always run unchecked but can take a positive turn. Similarly, there could be a term to assess damping, the opposite of repair.

Once we had the equations for a couple, the modeling allowed us to simulate marital interaction under different contexts, ones in which we had never observed the couple. This simulation was a first in the interaction field. For example, we could use the equations to ask what the interaction in this marriage might be like if the husband or wife started off very positively. This ability to simulate led naturally to experimentation to obtain proximal (not distal) changes in marital interaction. Most clinical trials are complex, multicomponent interventions designed to create large effects in the marriage. However, we were led to the idea of doing very clear and simple interventions designed to change parameters in the mathematical model for the second of two conversations a couple had in our laboratory. That is, rather than doing a clinical trial to change the whole marriage, we could try to change the couple's pattern of interaction only in the next conversation. We called these interventions our "marriage experiments." For example, we could see what effects just lowering a couple's heart rates or conversely, increasing their heart rates, would have onthe next conversation. Because these experiments were not clinical trials, but "exercises," we could manipulate an intervention variable by causing it to increase or decrease or even by leaving it unchanged. We were able to bring marriage into the social psychology laboratory for precise study.

Our pilot research on these marriage experiments was surprising because we found that we could change marital interaction quite dramatically for a brief time with very simple interventions. Previous clinical trials, when effective, often resulted in little gain in understanding (the interventions were so complex that it was difficult for investigators even to agree on what were the active ingredients of the change). On the other hand, armed with the mathematical model, we could do real experiments, very simple ones, and evaluate which processes our interventions were affecting. We are just at the threshold of this research.

It is time now for us to attempt to share our processes and methods with other researchers. We have had success in publishing our ideas and in explaining them clearly enough to a grant review panel clearly enough to be funded to do the marriage experiments. Hence, despite a pervasive math phobia among many of us, we wrote this book with a great deal of optimism. We are optimistic in part because for four years Gottman and Murray haved taught a very successful seminar jointly between the applied mathematics and psychology departments. The seminar brings graduate students in applied mathematics together with graduate students in psychology. The mathematicians have learned to become consultants, guided by James Murray, and the psychologists have learned to formulate their ideas more formally, guided by John Gottman.

We believe that our work represents the missing step necessary to complete the seminal thinking that the family general systems theorists started in the 1950s. We believe that this thinking is enriched by the mathematics we explain in this book- ideas such as catastrophe theory.

We revisit ideas such as homeostasis, developing them with concepts of phase space, null clines, influence functions, inertia, and uninfluenced and influenced stable steady states (attractors). We hope that these ideas will now be accessible to researchers who can weight their data with positive and negative weights, as we have done with Gottman's Specific Affect Coding System (SPAFF). Most coding systems in marriage are amenable to this weighting. These data already exist in most marriage research labs, so it is well within our readers' power to try these methods on their own. We also offer our computer programs for doing the computations.

Although the models are mathematical in the sense that they involve equations and mathematical concepts, the mathematical expertise required to employ these methods is minimal. Details of the mathematics are avoided in this book (or relegated to appendices) so that reader's can fully understand the models and the modeling process, the ideas and the results.

The process is seductive because we think what we will ultimately gain is understanding.

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Personal Preface: John Gottman

I want to give the reader a sense of what an amazing gift of fortune it was for me to meet James Murray. I have a background in mathematics (an undergraduate major at Fairleigh Dickinson University, and a master's degree in mathematics from MIT.). When my wife was pregnant with our daughter in 1990 we took a trip to Yellowstone Park, and we had some of those rare great heart-to-heart talks that people can have only on a long car ride. I told her that I had come to the view that I could not respect my own work unless I could somehow make it theoretical and mathematical. I felt that I had to return to my childhood roots as a mathematician. I had a sense that this new route would lead me to explore various mathematical methods for the study of change, so I later taught a seminar on the analysis of change. As part of that seminar, I explored nonlinear differential equations and chaos theory, but the exploration left me very confused.

I had read probably fifty books on nonlinear dynamics and chaos before I read James Murray's book *Mathematical Biology*. The other books left the process of modeling mystical and somewhat romantic. I had no real idea how to apply these methods to my data. Fortunately I was in a book club that selected James' book, and once I started reading it, my eyes were opened. It contained example after example with real problems and real data. I began to see that it was possible to demystify these marvelous methods and bring them into my own laboratory. I wrote a letter to Oxford University (where the book said James lived) to invite James to come and talk in my seminar; a few weeks later my program coordinator, Sharon Fentiman, received a call from James. When she remarked that the connection was so clear for an international call, he said that he was at the University of Washington. She asked him how long he would be in town, and he told her that he had moved to Seattle. What an amazing coincidence! We arranged to meet at the Faculty Club with my graduate student Regina Rushe. We hit it off instantly James talked in my seminar, and at lunch one of the students suggested that James and I fit together so well that we ought to teach a course together. We looked at one another and decided to do just that. By the kindness of fate, neither of us had to teach very much at all, but we elected to take this on. What a joy it was, and a lifelong friendship began.

Aside from a sense of this friendship, which in my life has always been the seed of fruitful collaboration, I also want to give the reader a personal recollection of the modeling process. My goal here is to suggest that, in hindsight, it is easy to create models that enrich a scientific enterprise and give it shape and direction. When we first sat down with James and his students, they asked us questions about the "laws" of marital relationships that would guide us in writing down the equations. We shrugged and said that there were no laws, no real guides. But there were some phenomena I could suggest might be replicable findings. I pointed out the phenomenon of "the triumph of negative over positive affect," which was the reliable finding, at that point, that negative affect was a better discriminator of happy from unhappy couples than positive affect and a better predictor of marital outcomes. Eventually we were to discover the 5 to 1 ratio of positive to negative affect in stable couples and the 0.8 to 1 ratio in couples heading for divorce. We learned how to measure positive affect more accurately, and its role became much more important. Nonetheless, the phenomenon held. Negative affect was far more destructive than positive affect was constructive. Other phenomena also emerged (and were discovered) as we met and talked, including Fritz Heider's "fundamental attribution error," in which people in ailing marriages eventually blame their partners for their troubles and see small negative events and brief changes for the worse as owing to lasting defects in their partners' character, but are quite forgiving toward themselves. We decided that we needed to address the dimension of power if we were to develop an adequate model of the unfolding of mutual influence over time. The act of trying to write down equations and to build a model began to shape my thinking about my own work.

Very gradually Julian Cook (one of James' students), Jane White, Rebecca Tyson, and I started examining data to guide us in the choice of model. We all decided that we could dismantle a couple's behavior on our "point graphs" that divided marital outcome into self-influences, and other influences, and other influences. When way we tried dismantling the data, it did not work until we included a term for each person's starting point, the constant that each person brought to the interaction. Then the model fit the data. As soon as we were able to write down the model, we began teaching one another what the components of the model meant. We realized then that we had a new language for talking about interaction, one that was breathtaking in its simplicity and power. For the first time, systems theory concepts were not metaphors but something real and potentially disconfirmable. The model could grow and develop.

The model not only fit the data, but also led to a new discovery: the mismatch between spouses in influence functions predicted divorce. That discovery was entirely unexpected. For some couples, there were two attractors, a positive and a negative attractor, which meant that the concept of family "homeostasis" had to be modified to include more than one such attractor.

As we began using the model, questions arose, which is James' criterion for a good model. What should be the theoretical shape of the influence functions? If we used the bilinear model, perhaps all couples would theoretically have two homeostatic set points, a positive and a negative one. What would be the implications of that type of model? Could we measure the "strength" of an attractor, considering that it might be what changed after an intervention? Could we make the model less static and include a repair term? If there were a repair term, was there also an analogous damping term? If we were able to model interaction so well, what about our perception variable, the video recall rating dial that people turned when they watched their videotape and told us what they were feeling during the interaction? What about physiology? All three domains for two people were time synchronized, so could we build models for all three domains? What about models across domains and their predictive power? Could we describe anything fundamental about how interactive behavior, perception, and physiology were related for marriages with respect to predicting marital outcomes?

All this became possible once we wrote down our equations and sat back, and reflected on what they meant. In hindsight, it all seems easy, and we wrote this book to convince our readers to try the same process for their own data. It was a journey well worth taking.

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